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I.—STUDIES IN THE LOGIC OF CONFIRMATION (I.).

To the memory of my wife, Eva Ahrends Hempel.

BY CARL G. HEMPEL.

1. *Objective of the Study.*[1]—The defining characteristic of an empirical statement is its capability of being tested by a confrontation with experimental findings, *i.e.* with the results of suitable experiments or "focussed" observations. This feature distinguishes statements which have empirical content both from the statements of the formal sciences, logic and mathematics, which require no experiential test for their validation, and from

[1] The present analysis of confirmation was to a large extent suggested and stimulated by a co-operative study of certain more general problems which were raised by Dr. Paul Oppenheim, and which I have been investigating with him for several years. These problems concern the form and the function of scientific laws and the comparative methodology of the different branches of empirical science. The discussion with Mr. Oppenheim of these issues suggested to me the central problem of the present essay. The more comprehensive problems just referred to will be dealt with by Mr. Oppenheim in a publication which he is now preparing.

In my occupation with the logical aspects of confirmation, I have benefited greatly by discussions with several students of logic, including Professor R. Carnap, Professor A. Tarski, and particularly Dr. Nelson Goodman, to whom I am indebted for several valuable suggestions which will be indicated subsequently.

A detailed exposition of the more technical aspects of the analysis of confirmation presented in this article is included in my article "A Purely Syntactical Definition of Confirmation", *The Journal of Symbolic Logic*, vol. 8 (1943).

the formulations of transempirical metaphysics, which do not admit of any.

The testability here referred to has to be understood in the comprehensive sense of "testability in principle"; there are many empirical statements which, for practical reasons, cannot be actually tested at present. To call a statement of this kind testable in principle means that it is possible to state just what experiential findings, if they were actually obtained, would constitute favourable evidence for it, and what findings or "data", as we shall say for brevity, would constitute unfavourable evidence; in other words, a statement is called testable in principle, if it is possible to describe the kind of data which would confirm or disconfirm it.

The concepts of confirmation and of disconfirmation as here understood are clearly more comprehensive than those of conclusive verification and falsification. Thus, *e.g.* no finite amount of experiential evidence can conclusively verify a hypothesis expressing a general law such as the law of gravitation, which covers an infinity of potential instances, many of which belong either to the as yet inaccessible future, or to the irretrievable past; but a finite set of relevant data may well be "in accord with" the hypothesis and thus constitute confirming evidence for it. Similarly, an existential hypothesis, asserting, say, the existence of an as yet unknown chemical element with certain specified characteristics, cannot be conclusively proved false by a finite amount of evidence which fails to "bear out" the hypothesis; but such unfavourable data may, under certain conditions, be considered as weakening the hypothesis in question, or as constituting disconfirming evidence for it.[1]

While, in the practice of scientific research, judgments as to the confirming or disconfirming character of experiential data obtained in the test of a hypothesis are often made without hesitation and with a wide consensus of opinion, it can hardly be said that these judgments are based on an explicit theory providing general criteria of confirmation and of disconfirmation. In this respect, the situation is comparable to the manner in which deductive inferences are carried out in the practice of scientific research: This, too, is often done without reference to an explicitly stated system of rules of logical inference. But while criteria of valid deduction can be and have been supplied by

[1] This point as well as the possibility of conclusive verification and conclusive falsification will be discussed in some detail in section 10 of the present paper.

formal logic, no satisfactory theory providing general criteria of confirmation and disconfirmation appears to be available so far.

In the present essay, an attempt will be made to provide the elements of a theory of this kind. After a brief survey of the significance and the present status of the problem, I propose to present a detailed critical analysis of some common conceptions of confirmation and disconfirmation and then to construct explicit definitions for these concepts and to formulate some basic principles of what might be called the logic of confirmation.

2. *Significance and Present Status of the Problem.*—The establishment of a general theory of confirmation may well be regarded as one of the most urgent desiderata of the present methodology of empirical science.[1] Indeed, it seems that a precise analysis of the concept of confirmation is a necessary condition for an adequate solution of various fundamental problems concerning the logical structure of scientific procedure. Let us briefly survey the most outstanding of these problems.

(*a*) In the discussion of scientific method, the concept of relevant evidence plays an important part. And while certain "inductivist" accounts of scientific procedure seem to assume that relevant evidence, or relevant data, can be collected in the context of an inquiry prior to the formulation of any hypothesis, it should be clear upon brief reflection that relevance is a relative concept; experiential data can be said to be relevant or irrelevant only with respect to a given hypothesis; and it is the hypothesis which determines what kind of data or evidence are relevant for it. Indeed, an empirical finding is relevant for a hypothesis if and only if it constitutes either favourable or unfavourable evidence for it; in other words, if it either confirms or disconfirms the hypothesis. Thus, a precise definition of relevance presupposes an analysis of confirmation and disconfirmation.

(*b*) A closely related concept is that of instance of a hypothesis. The so-called method of inductive inference is usually presented as proceeding from specific cases to a general hypothesis of which each of the special cases is an "instance" in the sense that it "conforms to" the general hypothesis in question, and thus constitutes confirming evidence for it.

Thus, any discussion of induction which refers to the establishment of general hypotheses on the strength of particular instances is fraught with all those logical difficulties—soon to be expounded

[1] Or of the "logic of science", as understood by R. Carnap; *cf. The Logical Syntax of Language* (New York and London, 1937), sect. 72, and the supplementary remarks in *Introduction to Semantics* (Cambridge, Mass., 1942), p. 250.

—which beset the concept of confirmation. A precise analysis of this concept is, therefore, a necessary condition for a clear statement of the issues involved in the problem complex of induction and of the ideas suggested for their solution—no matter what their theoretical merits or demerits may be.

(*c*) Another issue customarily connected with the study of scientific method is the quest for "rules of induction". Generally speaking, such rules would enable us to "infer", from a given set of data, that hypothesis or generalization which accounts best for all the particular data in the given set. Recent logical analyses have made it increasingly clear that this way of conceiving the problem involves a misconception: While the process of invention by which scientific discoveries are made is as a rule *psychologically guided and stimulated* by antecedent knowledge of specific facts, its results are *not logically determined* by them; the way in which scientific hypotheses or theories are discovered cannot be mirrored in a set of general rules of inductive inference.[1] One of the crucial considerations which lead to this conclusion is the following: Take a scientific theory such as the atomic theory of matter. The evidence on which it rests may be described in terms referring to directly observable phenomena, namely to certain "macroscopic" aspects of the various experimental and observational data which are relevant to the theory. On the other hand, the theory itself contains a large number of highly abstract, non-observational terms such as "atom", "electron", "nucleus", "dissociation", "valence" and others, none of which figures in the description of the observational data. An adequate rule of induction would therefore have to provide, for this and for every conceivable other case, mechanically applicable criteria determining unambiguously, and without any reliance on the inventiveness or additional scientific knowledge of its user, all those new abstract concepts which need to be created for the formulation of the theory that will account for the given evidence. Clearly, this requirement cannot be satisfied by any set of rules, however ingeniously devised; there can be no general rules of induction in the above sense; the demand for them rests on a confusion of logical and psychological issues. What determines the soundness of a hypothesis is not the way it

[1] See the lucid presentation of this point in Karl Popper's *Logik der Forschung* (Wien, 1935), esp. sect. 1, 2, 3, and 25, 26, 27; *cf.* also Albert Einstein's remarks in his lecture *On the Method of Theoretical Physics* (Oxford, 1933,) pp. 11 and 12. Also of interest in this context is the critical discussion of induction by H. Feigl in "The Logical Character of the Principle of Induction," *Philosophy of Science*, vol. 1 (1934).

is arrived at (it may even have been suggested by a dream or a hallucination), but the way it stands up when tested, *i.e.* when confronted with relevant observational data. Accordingly, the quest for rules of induction in the original sense of canons of scientific discovery has to be replaced, in the logic of science, by the quest for general objective criteria determining (A) whether, and—if possible—even (B) to what degree, a hypothesis H may be said to be corroborated by a given body of evidence E. This approach differs essentially from the inductivist conception of the problem in that it presupposes not only E, but also H as given and then seeks to determine a certain logical relationship between them. The two parts of this latter problem can be restated in somewhat more precise terms as follows:

(A) To give precise definitions of the two non-quantitative relational concepts of confirmation and of disconfirmation; *i.e.* to define the meaning of the phrases "E confirms H" and "E disconfirms H". (When E neither confirms nor disconfirms H, we shall say that E is neutral, or irrelevant, with respect to H.)

(B) (1) To lay down criteria defining a metrical concept "degree of confirmation of H with respect to E", whose values are real numbers; or, failing this,

(2) To lay down criteria defining two relational concepts, "more highly confirmed than" and "equally well confirmed with", which make possible a non-metrical comparison of hypotheses (each with a body of evidence assigned to it) with respect to the extent of their confirmation.

Interestingly, problem B has received much more attention in methodological research than problem A; in particular, the various theories of the "probability of hypotheses" may be regarded as concerning this problem complex; we have here adopted [1] the more neutral term "degree of confirmation" instead of "probability" because the latter is used in science in a definite technical sense involving reference to the relative frequency of the occurrence of a given event in a sequence, and it is at least an open question whether the degree of confirmation of a hypothesis can generally be defined as a probability in this statistical sense.

The theories dealing with the probability of hypotheses fall into two main groups: the "logical" theories construe probability as a logical relation between sentences (or propositions;

[1] Following R. Carnap's usage in Testability and Meaning, *Philosophy of Science*, vols. 3 (1936) and 4 (1937); esp. sect. 3 (in vol. 3).

it is not always clear which is meant)[1]; the "statistical" theories interpret the probability of a hypothesis in substance as the limit of the relative frequency of its confirming instances among all relevant cases.[2] Now it is a remarkable fact that none of the theories of the first type which have been developed so far provides an explicit general definition of the probability (or degree of confirmation) of a hypothesis H with respect to a body of evidence E; they all limit themselves essentially to the construction of an uninterpreted postulational system of logical probability. For this reason, these theories fail to provide a complete solution of problem B. The statistical approach, on the other hand, would, if successful, provide an explicit numerical definition of the degree of confirmation of a hypothesis; this definition would be formulated in terms of the numbers of confirming and disconfirming instances for H which constitute the body of evidence E. Thus, a necessary condition for an adequate interpretation of degrees of confirmation as statistical probabilities is the establishment of precise criteria of confirmation and disconfirmation, in other words, the solution of problem A.

However, despite their great ingenuity and suggestiveness, the attempts which have been made so far to formulate a precise statistical definition of the degree of confirmation of a hypothesis seem open to certain objections,[3] and several authors[4] have expressed doubts as to the possibility of defining the degree of confirmation of a hypothesis as a metrical magnitude, though

[1] This group includes the work of such writers as Janina Hosiasson-Lindenbaum (*cf.* for instance, her article "Induction et analogie: Comparaison de leur fondement", MIND, vol. L (1941); also see p. 21, n. 2), H. Jeffreys, J. M. Keynes, B. O. Koopman, J. Nicod (see p. 9, n. 2), St. Mazurkiewicz, F. Waismann. For a brief discussion of this conception of probability, see Ernest Nagel, *Principles of the Theory of Probability* (Internat. Encyclopedia of Unified Science, vol. i, no. 6, Chicago, 1939), esp. sects. 6 and 8.

[2] The chief proponent of this view is Hans Reichenbach; *cf.* especially Ueber Induktion und Wahrscheinlichkeit, *Erkenntnis*, vol. v (1935), and *Experience and Prediction* (Chicago, 1938), Ch. V.

[3] *Cf.* Karl Popper, *Logik der Forschung* (Wien, 1935), sect. 80; Ernest Nagel, *l.c.*, sect. 8, and "Probability and the Theory of Knowledge", *Philosophy of Science*, vol. 6 (1939); C. G. Hempel, "Le problème de la vérité", *Theoria* (Göteborg), vol. 3 (1937), sect. 5, and "On the Logical Form of Probability Statements", *Erkenntnis*, vol. 7 (1937-38), esp. sect. 5. *Cf.* also Morton White, "Probability and Confirmation", *The Journal of Philosophy*, vol. 36 (1939).

[4] See, for example, J. M. Keynes, *A Treatise on Probability*, London, 1929, esp. Ch. III; Ernest Nagel, *Principles of the Theory of Probability* (*cf.* n. 1 above), esp. p. 70; compare also the somewhat less definitely sceptical statement by Carnap, *l.c.* (see p. 5, n. 1), sect. 3, p. 427.

some of them consider it as possible, under certain conditions, to solve at least the less exacting problem B (2), *i.e.* to establish standards of non-metrical comparison between hypotheses with respect to the extent of their confirmation. An adequate comparison of this kind might have to take into account a variety of different factors[1]; but again the numbers of the confirming and of the disconfirming instances which the given evidence includes will be among the most important of those factors.

Thus, of the two problems, A and B, the former appears to be the more basic one, first, because it does not presuppose the possibility of defining numerical degrees of confirmation or of comparing different hypotheses as to the extent of their confirmation; and second because our considerations indicate that any attempt to solve problem B—unless it is to remain in the stage of an axiomatized system without interpretation—is likely to require a precise definition of the concepts of confirming and disconfirming instance of a hypothesis before it can proceed to define numerical degrees of confirmation, or to lay down non-metrical standards of comparison.

(*d*) It is now clear that an analysis of confirmation is of fundamental importance also for the study of the central problem of what is customarily called epistemology; this problem may be characterized as the elaboration of "standards of rational belief" or of criteria of warranted assertibility. In the methodology of empirical science this problem is usually phrased as concerning the rules governing the test and the subsequent acceptance or rejection of empirical hypotheses on the basis of experimental or observational findings, while in its "epistemological" version the issue is often formulated as concerning the validation of beliefs by reference to perceptions, sense data, or the like. But no matter how the final empirical evidence is construed and in what terms it is accordingly expressed, the theoretical problem remains the same: to characterize, in precise and general terms, the conditions under which a body of evidence can be said to confirm, or to disconfirm, a hypothesis of empirical character; and that is again our problem A.

(*e*) The same problem arises when one attempts to give a precise statement of the empiricist and operationalist criteria for the empirical meaningfulness of a sentence; these criteria, as is well known, are formulated by reference to the theoretical

[1] See especially the survey of such factors given by Ernest Nagel in *Principles of the Theory of Probability* (*cf.* p. 6, n. 1), pp. 66-73.

testability of the sentence by means of experiential evidence [1]; and the concept of theoretical testability, as was pointed out earlier, is closely related to the concepts of confirmation and disconfirmation.[2]

Considering the great importance of the concept of confirmation, it is surprising that no systematic theory of the non-quantitative relation of confirmation seems to have been developed so far. Perhaps this fact reflects the tacit assumption that the concepts of confirmation and of disconfirmation have a sufficiently clear meaning to make explicit definitions unnecessary or at least comparatively trivial. And indeed, as will be shown below, there are certain features which are rather generally associated with the intuitive notion of confirming evidence, and which, at first, seem well suited to serve as defining characteristics of confirmation. Closer examination will reveal the definitions thus obtainable to be seriously deficient and will make it clear that an adequate definition of confirmation involves considerable difficulties.

Now the very existence of such difficulties suggests the question whether the problem we are considering does not rest on a false assumption: Perhaps there are no objective criteria of confirmation; perhaps the decision as to whether a given hypothesis is acceptable in the light of a given body of evidence is no more subject to rational, objective rules than is the process of inventing a scientific hypothesis or theory; perhaps, in the last analysis, it is a "sense of evidence", or a feeling of plausibility in view of the relevant data, which ultimately decides whether a hypothesis is scientifically acceptable.[3] This view is comparable to the opinion that the validity of a mathematical proof or of a logical argument has to be judged ultimately by reference to a feeling of soundness or convincingness; and both theses have to be rejected on analogous grounds: They involve a confusion of logical and psychological considerations. Clearly, the occurrence

[1] *Cf.*, for example, A. J. Ayer, *Language, Truth and Logic*, London and New York, 1936, Ch. I; R. Carnap, "Testability and Meaning" (*cf.* p. 5, n. 1) sects. 1, 2, 3; H. Feigl, *Logical Empiricism* (in *Twentieth Century Philosophy*, ed. by Dagobert D. Runes, New York, 1943); P. W. Bridgman, *The Logic of Modern Physics*, New York, 1928.

[2] It should be noted, however, that in his essay "Testability and Meaning" (*cf.* p. 5, n. 1) R. Carnap has constructed definitions of testability and confirmability which avoid reference to the concept of confirming and of disconfirming evidence; in fact, no proposal for the definition of these latter concepts is made in that study.

[3] A view of this kind has been expressed, for example, by M. Mandelbaum in "Causal Analyses in History", *Journal of the History of Ideas*, vol. 3 (1942); *cf.* esp. pp. 46-47.

or non-occurrence of a feeling of conviction upon the presentation of grounds for an assertion is a subjective matter which varies from person to person, and with the same person in the course of time; it is often deceptive, and can certainly serve neither as a necessary nor as a sufficient condition for the soundness of the given assertion.[1] A rational reconstruction of the standards of scientific validation cannot, therefore, involve reference to a sense of evidence; it has to be based on objective criteria. In fact, it seems reasonable to require that the criteria of empirical confirmation, besides being objective in character, should contain no reference to the specific subject-matter of the hypothesis or of the evidence in question; it ought to be possible, one feels, to set up purely formal criteria of confirmation in a manner similar to that in which deductive logic provides purely formal criteria for the validity of deductive inferences.

With this goal in mind, we now turn to a study of the non-quantitative concept of confirmation. We shall begin by examining some current conceptions of confirmation and exhibiting their logical and methodological inadequacies; in the course of this analysis, we shall develop a set of conditions for the adequacy of any proposed definition of confirmation; and finally, we shall construct a definition of confirmation which satisfies those general standards of adequacy.

3. *Nicod's Criterion of Confirmation and its Shortcomings.*—We consider first a conception of confirmation which underlies many recent studies of induction and of scientific method. A very explicit statement of this conception has been given by Jean Nicod in the following passage: "Consider the formula or the law: *A entails B*. How can a particular proposition, or more briefly, a fact, affect its probability? If this fact consists of the presence of B in a case of A, it is favourable to the law '*A entails B*'; on the contrary, if it consists of the absence of B in a case of A, it is unfavourable to this law. It is conceivable that we have here the only two direct modes in which a fact can influence the probability of a law. . . . Thus, the entire influence of particular truths or facts on the probability of universal propositions or laws would operate by means of these two elementary relations which we shall call *confirmation* and *invalidation*."[2] Note that the applicability of this criterion is restricted to hypotheses of

[1] See Karl Popper's pertinent statement, *l.c.*, sect. 8.

[2] Jean Nicod, *Foundations of Geometry and Induction* (transl. by P. P. Wiener), London, 1930; p. 219; *cf.* also R. M. Eaton's discussion of "Confirmation and Infirmation", which is based on Nicod's views; it is included in Ch. III of his *General Logic*, New York, 1931.

the form "*A entails B*". Any hypothesis *H* of this kind may be expressed in the notation of symbolic logic [1] by means of a universal conditional sentence, such as, in the simplest case,

$$(x)(P(x) \supset Q(x)),$$

i.e. "For any object x: if x is a P, then x is a Q," or also "Occurrence of the quality P entails occurrence of the quality Q." According to the above criterion this hypothesis is confirmed by an object a, if a is P and Q; and the hypothesis is disconfirmed by a if a is P, but not Q. In other words, an object confirms a universal conditional hypothesis if and only if it satisfies both the antecedent (here: '$P(x)$') and the consequent (here: '$Q(x)$') of the conditional; it disconfirms the hypothesis if and only if it satisfies the antecedent, but not the consequent of the conditional; and (we add this to Nicod's statement) it is neutral, or irrelevant, with respect to the hypothesis if it does not satisfy the antecedent.

This criterion can readily be extended so as to be applicable also to universal conditionals containing more than one quantifier, such as "Twins always resemble each other", or, in symbolic notation, '$(x)(y)(\text{Twins}(x, y) \supset \text{Rsbl}(x, y))$'. In these cases, a confirming instance consists of an ordered couple, or triple, etc., of objects satisfying the antecedent and the consequent of the conditional. (In the case of the last illustration, any two persons who are twins and resemble each other would confirm the hypothesis; twins who do not resemble each other would disconfirm it; and any two persons not twins—no matter whether they resemble each other or not—would constitute irrelevant evidence.)

We shall refer to this criterion as Nicod's criterion.[2] It states explicitly what is perhaps the most common tacit interpretation of the concept of confirmation. While seemingly quite adequate, it suffers from serious shortcomings, as will now be shown.

(*a*) First, the applicability of this criterion is restricted to hypotheses of universal conditional form; it provides no standards of confirmation for existential hypotheses (such as "There exists organic life on other stars", or "Poliomyelitis is caused by some virus") or for hypotheses whose explicit formulation calls for the use of both universal and existential quantifiers (such as

[1] In this paper, only the most elementary devices of this notation are used; the symbolism is essentially that of *Principia Mathematica*, except that parentheses are used instead of dots, and that existential quantification is symbolized by '(E)' instead of by the inverted 'E'.

[2] This term is chosen for convenience, and in view of the above explicit formulation given by Nicod; it is not, of course, intended to imply that this conception of confirmation originated with Nicod.

"Every human being dies some finite number of years after his birth", or the psychological hypothesis, "You can fool all of the people some of the time and some of the people all of the time, but you cannot fool all of the people all of the time", which may be symbolized by '$(x)(Et)\mathrm{Fl}(x, t) \,.\, (Ex)(t)\mathrm{Fl}(x, t) \,.\, \sim (x)(t)\mathrm{Fl}(x, t)$', (where '$\mathrm{Fl}(x, t)$' stands for "You can fool (person) x at time t"). We note, therefore, the desideratum of establishing a criterion of confirmation which is applicable to hypotheses of any form.[1]

(*b*) We now turn to a second shortcoming of Nicod's criterion. Consider the two sentences

$$S_1 : `(x)(\mathrm{Raven}(x) \supset \mathrm{Black}(x))';$$
$$S_2 : `(x)(\sim \mathrm{Black}(x) \supset \sim \mathrm{Raven}(x))'$$

(*i.e.* "All ravens are black" and "Whatever is not black is not a raven"), and let a, b, c, d be four objects such that a is a raven and black, b a raven but not black, c not a raven but black, and d neither a raven nor black. Then, according to Nicod's criterion, a would confirm S_1, but be neutral with respect to S_2; b would disconfirm both S_1 and S_2; c would be neutral with respect to both S_1 and S_2, and d would confirm S_2, but be neutral with respect to S_1.

But S_1 and S_2 are logically equivalent; they have the same content, they are different formulations of the same hypothesis. And yet, by Nicod's criterion, either of the objects a and d would be confirming for one of the two sentences, but neutral with respect to the other. This means that Nicod's criterion makes confirmation depend not only on the content of the hypothesis, but also on its formulation.[2]

One remarkable consequence of this situation is that every hypothesis to which the criterion is applicable—*i.e.* every universal conditional—can be stated in a form for which there cannot possibly exist any confirming instances. Thus, *e.g.* the sentence

$$(x)[(\mathrm{Raven}(x) \,.\, \sim \mathrm{Black}(x)) \supset (\mathrm{Raven}(x) \,.\, \sim \mathrm{Raven}(x)]$$

is readily recognized as equivalent to both S_1 and S_2 above; yet no object whatever can confirm this sentence, *i.e.* satisfy both

[1] For a rigorous formulation of the problem, it is necessary first to lay down assumptions as to the means of expression and the logical structure of the language in which the hypotheses are supposed to be formulated; the desideratum then calls for a definition of confirmation applicable to any hypothesis which can be expressed in the given language. Generally speaking, the problem becomes increasingly difficult with increasing richness and complexity of the assumed "language of science".

[2] This difficulty was pointed out, in substance, in my article "Le problème de la vérité", *Theoria* (Göteborg), vol. 3 (1937), esp. p. 222.

its antecedent and its consequent; for the consequent is contradictory. An analogous transformation is, of course, applicable to any other sentence of universal conditional form.

4. *The Equivalence Condition.*—The results just obtained call attention to a condition which an adequately defined concept of confirmation should satisfy, and in the light of which Nicod's criterion has to be rejected as inadequate: *Equivalence condition:* Whatever confirms (disconfirms) one of two equivalent sentences, also confirms (disconfirms) the other.

Fulfilment of this condition makes the confirmation of a hypothesis independent of the way in which it is formulated; and no doubt it will be conceded that this is a necessary condition for the adequacy of any proposed criterion of confirmation. Otherwise, the question as to whether certain data confirm a given hypothesis would have to be answered by saying: "That depends on which of the different equivalent formulations of the hypothesis is considered"—which appears absurd. Furthermore—and this is a more important point than an appeal to a feeling of absurdity—an adequate definition of confirmation will have to do justice to the way in which empirical hypotheses function in theoretical scientific contexts such as explanations and predictions; but when hypotheses are used for purposes of explanation or prediction,[1] they serve as premisses in a deductive argument whose conclusion is a description of the event to be explained or predicted. The deduction is governed by the principles of formal logic, and according to the latter, a deduction which is valid will remain so if some or all of the premisses are replaced by different, but equivalent statements; and indeed, a scientist will feel free, in any theoretical reasoning involving certain hypotheses, to use the latter in whichever of their equivalent formulations is most convenient for the development of his conclusions. But if we adopted a concept of confirmation which did not satisfy the equivalence condition, then it would be possible, and indeed necessary, to argue in certain cases that it was sound scientific procedure to base a prediction on a given hypothesis if formulated in a sentence S_1, because a good deal of confirming evidence had

[1] For a more detailed account of the logical structure of scientific explanation and prediction, *cf.* C. G. Hempel, "The Function of General Laws in History", *The Journal of Philosophy*, vol. 39 (1942), esp. sects. 2, 3, 4. The characterization, given in that paper as well as in the above text, of explanations and predictions as arguments of a deductive logical structure, embodies an over-simplification: as will be shown in sect. 7 of the present essay, explanations and predictions often involve "quasi-inductive" steps besides deductive ones. This point, however, does not affect the validity of the above argument.

been found for S_1; but that it was altogether inadmissible to base the prediction (say, for convenience of deduction) on an equivalent formulation S_2, because no confirming evidence for S_2 was available. Thus, the equivalence condition has to be regarded as a necessary condition for the adequacy of any definition of confirmation.

5. *The "Paradoxes" of Confirmation.*—Perhaps we seem to have been labouring the obvious in stressing the necessity of satisfying the equivalence condition. This impression is likely to vanish upon consideration of certain consequences which derive from a combination of the equivalence condition with a most natural and plausible assumption concerning a sufficient condition of confirmation.

The essence of the criticism we have levelled so far against Nicod's criterion is that it certainly cannot serve as a necessary condition of confirmation; thus, in the illustration given in the beginning of section 3, the object a confirms S_1 and should therefore also be considered as confirming S_2, while according to Nicod's criterion it is not. Satisfaction of the latter is therefore not a necessary condition for confirming evidence.

On the other hand, Nicod's criterion might still be considered as stating a particularly obvious and important sufficient condition of confirmation. And indeed, if we restrict ourselves to universal conditional hypotheses in one variable [1]—such as S_1

[1] This restriction is essential: In its general form, which applies to universal conditionals in any number of variables, Nicod's criterion cannot even be construed as expressing a sufficient condition of confirmation. This is shown by the following rather surprising example: Consider the hypothesis S_1: $(x)(y)[\sim (R(x, y)) \supset (R(x, y) \,.\, \sim R(y, x))]$.

Let a, b be two objects such that $R(a, b)$ and $\sim R(b, a)$. Then clearly, the couple (a, b) satisfies both the antecedent and the consequent of the universal conditional S_1; hence, if Nicod's criterion in its general form is accepted as stating a sufficient condition of confirmation, (a, b) constitutes confirming evidence for S_1. However, S_1 can be shown to be equivalent to

$$S_2: (x)(y)R(x, y)$$

Now, by hypothesis, we have $\sim R(b, a)$; and this flatly contradicts S_2 and thus S_1. Thus, the couple (a, b), although satisfying both the antecedent and the consequent of the universal conditional S_1 actually constitutes disconfirming evidence of the strongest kind (conclusively disconfirming evidence, as we shall say later) for that sentence. This illustration reveals a striking and—as far as I am aware—hitherto unnoticed weakness of that conception of confirmation which underlies Nicod's criterion. In order to realize the bearing of our illustration upon Nicod's original formulation, let A and B be $\sim (R(x, y) \,.\, R(y, x))$ and $R(x, y) \,.\, \sim R(y, x)$ respectively. Then S_1 asserts that A entails B, and the couple (a, b) is a case of the presence of B in the presence of A; this should, according to Nicod, be favourable to S_1.

and S_2 in the above illustration—then it seems perfectly reasonable to qualify an object as confirming such a hypothesis if it satisfies both its antecedent and its consequent. The plausibility of this view will be further corroborated in the course of our subsequent analyses.

Thus, we shall agree that if a is both a raven and black, then a certainly confirms S_1: '(x) (Raven(x) $\supset$ Black(x))', and if d is neither black nor a raven, d certainly confirms S_2:

$$'(x)\,(\sim \text{Black}(x) \supset \sim \text{Raven}(x)).'$$

Let us now combine this simple stipulation with the equivalence condition: Since S_1 and S_2 are equivalent, d is confirming also for S_1; and thus, we have to recognize as confirming for S_1 any object which is neither black nor a raven. Consequently, any red pencil, any green leaf, and yellow cow, etc., becomes confirming evidence for the hypothesis that all ravens are black. This surprising consequence of two very adequate assumptions (the equivalence condition and the above sufficient condition of confirmation) can be further expanded: The following sentence can readily be shown to be equivalent to S_1: S_3: '(x) [(Raven(x) v $\sim$ Raven(x)) $\supset$ ($\sim$ Raven(x) v Black(x))]', *i.e.* "Anything which is or is not a raven is either no raven or black". According to the above sufficient condition, S_3 is certainly confirmed by any object, say e, such that (1) e is or is not a raven and, in addition, (2) e is not a raven or also black. Since (1) is analytic, these conditions reduce to (2). By virtue of the equivalence condition, we have therefore to consider as confirming for S_1 any object which is either no raven or also black (in other words: any object which is no raven at all, or a black raven).

Of the four objects characterized in section 3, a, c and d would therefore constitute confirming evidence for S_1, while b would be disconfirming for S_1. This implies that any non-raven represents confirming evidence for the hypothesis that all ravens are black.

We shall refer to these implications of the equivalence criterion and of the above sufficient condition of confirmation as the *paradoxes of confirmation*.

How are these paradoxes to be dealt with? Renouncing the equivalence condition would not represent an acceptable solution, as is shown by the considerations presented in section 4. Nor does it seem possible to dispense with the stipulation that an object satisfying two conditions, C_1 and C_2, should be considered as confirming a general hypothesis to the effect that any object which satisfies C_1, also satisfies C_2.

But the deduction of the above paradoxical results rests on

one other assumption which is usually taken for granted, namely, that the meaning of general empirical hypotheses, such as that all ravens are black, or that all sodium salts burn yellow, can be adequately expressed by means of sentences of universal conditional form, such as '(x) (Raven$(x) \supset$ Black(x))' and '(x) (Sod. Salt$(x) \supset$ Burn Yellow (x))', etc. Perhaps this customary mode of presentation has to be modified; and perhaps such a modification would automatically remove the paradoxes of confirmation? If this is not so, there seems to be only one alternative left, namely to show that the impression of the paradoxical character of those consequences is due to misunderstanding and can be dispelled, so that no theoretical difficulty remains. We shall now consider these two possibilities in turn: The sub-sections 5.11 and 5.12 are devoted to a discussion of two different proposals for a modified representation of general hypotheses; in subsection 5.2, we shall discuss the second alternative, *i.e.* the possibility of tracing the impression of paradoxicality back to a misunderstanding.

5.11. It has often been pointed out that while Aristotelian logic, in agreement with prevalent every day usage, confers "existential import" upon sentences of the form "All P's are Q's", a universal conditional sentence, in the sense of modern logic, has no existential import; thus, the sentence

'(x) (Mermaid$(x) \supset$ Green(x))'

does not imply the existence of mermaids; it merely asserts that any object either is not a mermaid at all, or a green mermaid; and it is true simply because of the fact that there are no mermaids. General laws and hypotheses in science, however—so it might be argued—are meant to have existential import; and one might attempt to express the latter by supplementing the customary universal conditional by an existential clause. Thus, the hypothesis that all ravens are black would be expressed by means of the sentence S_1: '(x) (Raven$(x) \supset$ Black(x)) . (Ex)Raven(x)'; and the hypothesis that no non-black things are ravens by S_2: '$(x)(\sim$ Black$(x) \supset \sim$ Raven$(x))$. $(Ex) \sim$ Black(x). Clearly, these sentences are not equivalent, and of the four objects a, b, c, d characterized in section 3, part (b), only a might reasonably be said to confirm S_1, and only d to confirm S_2. Yet this method of avoiding the paradoxes of confirmation is open to serious objections:

(a) First of all, the representation of every general hypothesis by a conjunction of a universal conditional and an existential sentence would invalidate many logical inferences which are

generally accepted as permissible in a theoretical argument. Thus, for example, the assertions that all sodium salts burn yellow, and that whatever does not burn yellow is no sodium salt are logically equivalent according to customary understanding and usage; and their representation by universal conditionals preserves this equivalence; but if existential clauses are added, the two assertions are no longer equivalent, as is illustrated above by the analogous case of S_1 and S_2.

(*b*) Second, the customary formulation of general hypotheses in empirical science clearly does not contain an existential clause, nor does it, as a rule, even indirectly determine such a clause unambiguously. Thus, consider the hypothesis that if a person after receiving an injection of a certain test substance has a positive skin reaction, he has diphtheria. Should we construe the existential clause here as referring to persons, to persons receiving the injection, or to persons who, upon receiving the injection, show a positive skin reaction? A more or less arbitrary decision has to be made; each of the possible decisions gives a different interpretation to the hypothesis, and none of them seems to be really implied by the latter.

(*c*) Finally, many universal hypotheses cannot be said to imply an existential clause at all. Thus, it may happen that from a certain astrophysical theory a universal hypothesis is deduced concerning the character of the phenomena which would take place under certain specified extreme conditions. A hypothesis of this kind need not (and, as a rule, does not) imply that such extreme conditions ever were or will be realized; it has no existential import. Or consider a biological hypothesis to the effect that whenever man and ape are crossed, the offspring will have such and such characteristics. This is a general hypothesis; it might be contemplated as a mere conjecture, or as a consequence of a broader genetic theory, other implications of which may already have been tested with positive results; but unquestionably the hypothesis does not imply an existential clause asserting that the contemplated kind of cross-breeding referred to will, at some time, actually take place.

While, therefore, the adjunction of an existential clause to the customary symbolization of a general hypothesis cannot be considered as an adequate *general* method of coping with the paradoxes of confirmation, there is a purpose which the use of an existential clause may serve very well, as was pointed out to me by Dr. Paul Oppenheim [1]: if somebody feels that objects of the

[1] This observation is related to Mr. Oppenheim's methodological studies referred to in p. 1, n. 1.

types c and d mentioned above are irrelevant rather than confirming for the hypothesis in question, and that qualifying them as confirming evidence does violence to the meaning of the hypothesis, then this may indicate that he is consciously or unconsciously construing the latter as having existential import; and this kind of understanding of general hypotheses is in fact very common. In this case, the "paradox" may be removed by pointing out that an adequate symbolization of the intended meaning requires the adjunction of an existential clause. The formulation thus obtained is more restrictive than the universal conditional alone; and while we have as yet set up no criteria of confirmation applicable to hypotheses of this more complex form, it is clear that according to every acceptable definition of confirmation objects of the types c and d will fail to qualify as confirming cases. In this manner, the use of an existential clause may prove helpful in distinguishing and rendering explicit different possible interpretations of a given general hypothesis which is stated in non-symbolic terms.

5.12. Perhaps the impression of the paradoxical character of the cases discussed in the beginning of section 5 may be said to grow out of the feeling that the hypothesis that all ravens are black is about ravens, and not about non-black things, nor about all things. The use of an existential clause was one attempt at expressing this presumed peculiarity of the hypothesis. The attempt has failed, and if we wish to reflect the point in question, we shall have to look for a stronger device. The idea suggests itself of representing a general hypothesis by the customary universal conditional, supplemented by the indication of the specific "field of application" of the hypothesis; thus, we might represent the hypothesis that all ravens are black by the sentence '(x) (Raven$(x) \supset$ Black(x))' (or any one of its equivalents), plus the indication "Class of ravens" characterizing the field of application; and we might then require that every confirming instance should belong to the field of application. This procedure would exclude the objects c and d from those constituting confirming evidence and would thus avoid those undesirable consequences of the existential-clause device which were pointed out in 5.11 (c). But apart from this advantage, the second method is open to objections similar to those which apply to the first: (a) The way in which general hypotheses are used in science never involves the statement of a field of application; and the choice of the latter in a symbolic formulation of a given hypothesis thus introduces again a considerable measure of arbitrariness. In particular, for a scientific hypothesis to the effect that

all *P*'s are *Q*'s, the field of application cannot simply be said to be the class of all *P*'s; for a hypothesis such as that all sodium salts burn yellow finds important applications in tests with negative results; *i.e.* it may be applied to a substance of which it is not known whether it contains sodium salts, nor whether it burns yellow; and if the flame does not turn yellow, the hypothesis serves to establish the absence of sodium salts. The same is true of all other hypotheses used for tests of this type. (*b*) Again, the consistent use of a domain of application in the formulation of general hypotheses would involve considerable logical complications, and yet would have no counterpart in the theoretical procedure of science, where hypotheses are subjected to various kinds of logical transformation and inference without any consideration that might be regarded as referring to changes in the fields of application. This method of meeting the paradoxes would therefore amount to dodging the problem by means of an *ad hoc* device which cannot be justified by reference to actual scientific procedure.

5.2. We have examined two alternatives to the customary method of representing general hypotheses by means of universal conditionals; neither of them proved an adequate means of precluding the paradoxes of confirmation. We shall now try to show that what is wrong does not lie in the customary way of construing and representing general hypotheses, but rather in our reliance on a misleading intuition in the matter: The impression of a paradoxical situation is not objectively founded; it is a psychological illusion.

(*a*) One source of misunderstanding is the view, referred to before, that a hypothesis of the simple form "Every *P* is a *Q*" such as "All sodium salts burn yellow", asserts something about a certain limited class of objects only, namely, the class of all *P*'s. This idea involves a confusion of logical and practical considerations: Our interest in the hypothesis may be focussed upon its applicability to that particular class of objects, but the hypothesis nevertheless asserts something about, and indeed imposes restrictions upon, *all* objects (within the logical type of the variable occurring in the hypothesis, which in the case of our last illustration might be the class of all physical objects). Indeed, a hypothesis of the form "Every *P* is a *Q*" forbids the occurrence of any objects having the property *P* but lacking the property *Q*; *i.e.* it restricts all objects whatsoever to the class of those which either lack the property *P* or also have the property *Q*. Now, every object either belongs to this class or falls outside it, and thus, every object—and not only the P's—either conforms to the

hypothesis or violates it; there is no object which is not implicitly "referred to" by a hypothesis of this type. In particular, every object which either is no sodium salt or burns yellow conforms to, and thus "bears out" the hypothesis that all sodium salts burn yellow; every other object violates that hypothesis.

The weakness of the idea under consideration is evidenced also by the observation that the class of objects about which a hypothesis is supposed to assert something is in no way clearly determined, and that it changes with the context, as was shown in 5.12 (*a*).

(*b*) A second important source of the appearance of paradoxicality in certain cases of confirmation is exhibited by the following consideration.

Suppose that in support of the assertion "All sodium salts burn yellow" somebody were to adduce an experiment in which a piece of pure ice was held into a colourless flame and did not turn the flame yellow. This result would confirm the assertion, "Whatever does not burn yellow is no sodium salt", and consequently, by virtue of the equivalence condition, it would confirm the original formulation. Why does this impress us as paradoxical? The reason becomes clear when we compare the previous situation with the case of an experiment where an object whose chemical constitution is as yet unknown to us is held into a flame and fails to turn it yellow, and where subsequent analysis reveals it to contain no sodium salt. This outcome, we should no doubt agree, is what was to be expected on the basis of the hypothesis that all sodium salts burn yellow—no matter in which of its various equivalent formulations it may be expressed; thus, the data here obtained constitute confirming evidence for the hypothesis. Now the only difference between the two situations here considered is that in the first case we are told beforehand the test substance is ice, and we happen to "know anyhow" that ice contains no sodium salt; this has the consequence that the outcome of the flame-colour test becomes entirely irrelevant for the confirmation of the hypothesis and thus can yield no new evidence for us. Indeed, if the flame should not turn yellow, the hypothesis requires that the substance contain no sodium salt—and we know beforehand that ice does not—and if the flame should turn yellow, the hypothesis would impose no further restrictions on the substance; hence, either of the possible outcomes of the experiment would be in accord with the hypothesis.

The analysis of this example illustrates a general point: In

the seemingly paradoxical cases of confirmation, we are often not actually judging the relation of the given evidence, *E* alone to the hypothesis *H* (we fail to observe the "methodological fiction", characteristic of every case of confirmation, that we have no relevant evidence for *H* other than that included in *E*); instead, we tacitly introduce a comparison of *H* with a body of evidence which consists of *E* in conjunction with an additional amount of information which we happen to have at our disposal; in our illustration, this information includes the knowledge (1) that the substance used in the experiment is ice, and (2) that ice contains no sodium salt. If we assume this additional information as given, then, of course, the outcome of the experiment can add no strength to the hypothesis under consideration. But if we are careful to avoid this tacit reference to additional knowledge (which entirely changes the character of the problem), and if we formulate the question as to the confirming character of the evidence in a manner adequate to the concept of confirmation as used in this paper, we have to ask: Given some object *a* (it happens to be a piece of ice, but this fact is not included in the evidence), and given the fact that *a* does not turn the flame yellow and is no sodium salt—does *a* then constitute confirming evidence for the hypothesis? And now—no matter whether *a* is ice or some other substance—it is clear that the answer has to be in the affirmative; and the paradoxes vanish.

So far, in section (*b*), we have considered mainly that type of paradoxical case which is illustrated by the assertion that any non-black non-raven constitutes confirming evidence for the hypothesis, "All ravens are black." However, the general idea just outlined applies as well to the even more extreme cases exemplified by the assertion that any non-raven as well as any black object confirms the hypothesis in question. Let us illustrate this by reference to the latter case. If the given evidence *E*—*i.e.* in the sense of the required methodological fiction, all our data relevant for the hypothesis—consists only of one object which, in addition, is black, then *E* may reasonably be said to support even the hypothesis that all objects are black, and *a fortiori* *E* supports the weaker assertion that all ravens are black. In this case, again, our factual knowledge that not all objects are black tends to create an impression of paradoxicality which is not justified on logical grounds. Other "paradoxical" cases of confirmation may be dealt with analogously, and it thus turns out that the "paradoxes of confirmation", as formulated above, are due to a misguided intuition in the matter rather than to a logical flaw

in the two stipulations from which the "paradoxes" were derived.[1], [2]

[1] The basic idea of sect. (*b*) in the above analysis of the "paradoxes of confirmation" is due to Dr. Nelson Goodman, to whom I wish to reiterate my thanks for the help he rendered me, through many discussions, in clarifying my ideas on this point.

[2] The considerations presented in section (*b*) above are also influenced by, though not identical in content with, the very illuminating discussion of the "paradoxes" by the Polish methodologist and logician Janina Hosiasson-Lindenbaum; *cf.* her article "On Confirmation", *The Journal of Symbolic Logic*, vol. 5 (1940), especially sect. 4. Dr. Hosiasson's attention had been called to the paradoxes by the article referred to in p. 11, n. 2, and by discussions with the author. To my knowledge, hers has so far been the only publication which presents an explicit attempt to solve the problem. Her solution is based on a theory of degrees of confirmation, which is developed in the form of an uninterpreted axiomatic system (*cf.* also p. 6, n. 1, and part (*b*) in sect. 1 of the present article), and most of her arguments presuppose that theoretical framework. I have profited, however, by some of Miss Hosiasson's more general observations which proved relevant for the analysis of the paradoxes of the non-gradated relation of confirmation which forms the object of the present study.

One point in those of Miss Hosiasson's comments which rest on her theory of degrees of confirmation is of particular interest, and I should like to discuss it briefly. Stated in reference to the raven-hypothesis, it consists in the suggestion that the finding of one non-black object which is no raven, while constituting confirming evidence for the hypothesis, would increase the degree of confirmation of the hypothesis by a smaller amount than the finding of one raven which is black. This is said to be so because the class of all ravens is much less numerous than that of all non-black objects, so that—to put the idea in suggestive though somewhat misleading terms—the finding of one black raven confirms a larger portion of the total content of the hypothesis than the finding of one non-black non-raven. In fact, from the basic assumptions of her theory, Miss Hosiasson is able to derive a theorem according to which the above statement about the relative increase in degree of confirmation will hold provided that actually the number of all ravens is small compared with the number of all non-black objects. But is this last numerical assumption actually warranted in the present case and analogously in all other "paradoxical" cases? The answer depends in part upon the logical structure of the language of science. If a "co-ordinate language" is used, in which, say, finite space-time regions figure as individuals, then the raven-hypothesis assumes some such form as "Every space-time region which contains a raven, contains something black"; and even if the total number of ravens ever to exist is finite, the class of space-time regions containing a raven has the power of the continuum, and so does the class of space-time regions containing something non-black; thus, for a co-ordinate language of the type under consideration, the above numerical assumption is not warranted. Now the use of a co-ordinate language may appear quite artificial in this particular illustration; but it will seem very appropriate in many other contexts, such as, *e.g.*, that of physical field theories. On the other hand, Miss Hosiasson's numerical assumption may well be justified on the basis of a "thing language", in which physical objects of finite size function

6. *Confirmation Construed as a Relation between Sentences.*—Our analysis of Nicod's criterion has so far led to two main results: The rejection of that criterion in view of several deficiencies, and the emergence of the equivalence condition as a necessary condition of adequacy for any proposed definition of confirmation. Another aspect of Nicod's criterion requires consideration now. In our formulation of the criterion, confirmation was construed as a dyadic relation between an object or an ordered set of objects, representing the evidence, and a sentence, representing the hypothesis. This means that confirmation was conceived of as a semantical relation [1] obtaining between certain extra-linguistic objects [2] on one hand and certain sentences on the other. It is possible, however, to construe confirmation in an alternative fashion as a relation between two sentences, one describing the given evidence, the other expressing the hypothesis. Thus, *e.g.* instead of saying that an object *a* which is both a raven and black (or the "fact" of *a* being both a raven and black) confirms the hypothesis, "All ravens are black", we may say that the evidence sentence, "*a* is a raven, and *a* is black", confirms the hypothesis-sentence (briefly, the hypothesis), "All ravens are black". We shall adopt this conception of confirmation as a relation between sentences here for the following reasons: First, the evidence adduced in support or criticism of a scientific hypothesis is always expressed in sentences, which frequently have the character of observation reports; and second, it will prove very fruitful to pursue the parallel, alluded to in section 2 above, between the concepts of confirmation and of logical consequence. And just as in the theory of the consequence relation, *i.e.* in deductive logic, the premisses of which a given conclusion is a consequence are construed as sentences rather than as "facts", so we propose to construe the data which confirm a given hypothesis as given in the form of sentences.

The preceding reference to observation reports suggests a certain restriction which might be imposed on evidence sentences. Indeed, the evidence adduced in support of a scientific hypothesis

as individuals. Of course, even on this basis, it remains an empirical question, for every hypothesis of the form "All *P*'s are *Q*'s", whether actually the class of non-*Q*'s is much more numerous than the class of *P*'s; and in many cases this question will be very difficult to decide.

[1] For a detailed account of this concept, see C. W. Morris, *Foundations of the Theory of Signs* (Internat. Encyclopedia of Unified Science, vol. i, no. 2, Chicago, 1938), and R. Carnap, *Introduction to Semantics* (Cambridge, Mass., 1942), esp. sects. 4 and 37.

[2] Instead of making the first term of the relation an object or a sequence of objects, we might construe it as a "state of affairs" (or perhaps as a "fact", or a "proposition", as Nicod puts it), such as that state of affairs which consists in *a* being a black raven, etc.

or theory consists, in the last analysis, in data accessible to what is loosely called " direct observation ", and such data are expressible in the form of " observation reports ". In view of this consideration, we shall restrict the evidence sentences which form the domain of the relation of confirmation, to sentences of the character of observation reports. In order to give a precise meaning to the concept of observation report, we shall assume that we are given a well-determined " language of science ", in terms of which all sentences under consideration, hypotheses as well as evidence sentences, are formulated. We shall further assume that this language contains, among other terms, a clearly delimited " observational vocabulary " which consists of terms designating more or less directly observable attributes of things or events, such as, say, " black ", " taller than ", " burning with a yellow light ", etc., but no theoretical constructs such as " aliphatic compound ", " circularly polarized light ", " heavy hydrogen ", etc.

We shall now understand by a hypothesis any sentence which can be expressed in the assumed language of science, no matter whether it is a generalized sentence, containing quantifiers, or a particular sentence referring only to a finite number of particular objects. An observation report will be construed as a finite class (or a conjunction of a finite number) of observation sentences; and an observation sentence as a sentence which either asserts or denies that a given object has a certain observable property (such as " a is a raven ", " d is not black "), or that a given sequence of objects stand in a certain observable relation (such as " a is between b and c ").

Now the concept of observability itself obviously is relative to the techniques of observation used. What is unobservable to the unaided senses may well be observable by means of suitable devices such as telescopes, microscopes, polariscopes, lie-detectors, Gallup-polls, etc. If by direct observation we mean such observational procedures as do not make use of auxiliary devices, then such property terms as " black ", " hard ", " liquid ", " cool ", and such relation terms as " above ", " between ", " spatially coincident ", etc., might be said to refer to directly observable attributes; if observability is construed in a broader sense, so as to allow for the use of certain specified instruments or other devices, the concept of observable attribute becomes more comprehensive. If, in our study of confirmation, we wanted to analyze the manner in which the hypotheses and theories of empirical science are ultimately supported by " evidence of the senses ", then we should have to require that observation reports refer exclusively to directly observable attributes. This view

was taken, for simplicity and concreteness, in the preceding parts of this section. Actually, however, the general logical characteristics of that relation which obtains between a hypothesis and a group of empirical statements which "support" it, can be studied in isolation from this restriction to direct observability. All we will assume here is that in the context of the scientific test of a given hypothesis or theory, certain specified techniques of observation have been agreed upon; these determine an observational vocabulary, namely a set of terms designating properties and relations observable by means of the accepted techniques. For our purposes it is entirely sufficient that these terms, constituting the "observational vocabulary", be given. An observation sentence is then defined simply as a sentence affirming or denying that a given object, or sequence of objects, possesses one of those observable attributes.[1]

Let it be noted that we do not require an observation sentence to be true, nor to be accepted on the basis of actual observations; rather, an observation sentence expresses something that is decidable by means of the accepted techniques of observation; in other words: An observation sentence describes a possible outcome of the accepted observational techniques; it asserts something that might conceivably be established by means of those

[1] The concept of observation sentence has, in the context of our study, a status and a logical function closely akin to that of the concepts of protocol statement or basis sentence, etc., as used in many recent studies of empiricism. However, the conception of observation sentence which is being proposed in the present study is more liberal in that it renders the discussion of the logical problems of testing and confirmation independent of various highly controversial epistemological issues; thus, *e.g.* we do not stipulate that observation reports must be about psychic acts, or about sense perceptions (*i.e.* that they have to be expressed in terms of a vocabulary of phenomenology, or of introspective psychology). According to the conception of observation sentence adopted in the present study, the "objects" referred to in an observation sentence may be construed in any one of the senses just referred to, or in various other ways; for example, they might be space-time regions, or again physical objects such as stones, trees, etc. (most of the illustrations given throughout this article represent observation sentences belonging to this kind of "thing-language"); all that we require is that the few very general conditions stated above be satisfied.

These conditions impose on observation sentences and on observation reports certain restrictions with respect to their form; in particular, neither kind of sentence may contain any quantifiers. This stipulation recommends itself for the purposes of the logical analysis here to be undertaken; but we do not wish to claim that this formal restriction is indispensable. On the contrary, it is quite possible and perhaps desirable also to allow for observation sentences containing quantifiers: our simplifying assumption is introduced mainly in order to avoid considerable logical complications in the definition of confirmation.

techniques. Possibly, the term "observation-type sentence" would be more suggestive; but for convenience we give preference to the shorter term. An analogous comment applies, of course, to our definition of an observation report as a class or a conjunction of observation sentences. The need for this broad conception of observation sentences and observation reports is readily recognized: Confirmation as here conceived is a logical relationship between sentences, just as logical consequence is. Now whether a sentence S_2 is a consequence of a sentence S_1 does not depend on whether S_1 is true (or known to be true), or not; and analogously, the criteria of whether a given statement, expressed in terms of the observational vocabulary, confirms a certain hypothesis cannot depend on whether the statements in the report are true, or based on actual experience, or the like. Our definition of confirmation must enable us to indicate what kind of evidence *would* confirm a given hypothesis *if* it were available; and clearly the sentence characterizing such evidence can be required only to express something that might be observed, but not necessarily something that has actually been established by obsrvation.

It may be helpful to carry the analogy between confirmation and consequence one step further. The truth or falsity of S_1 is irrelevant for the question of whether S_2 is a consequence of S_1 (whether S_2 can be validly inferred from S_1); but in a logical inference which justifies a sentence S_2 by showing that it is a logical consequence of a conjunction of premisses, S_1, we can be certain of the truth of S_2 only if we know S_1 to be true. Analogously, the question of whether an observation report stands in the relation of confirmation to a given hypothesis does not depend on whether the report states actual or fictitious observational findings; but for a decision as to the soundness or acceptability of a hypothesis which is confirmed by a certain report, it is of course necessary to know whether the report is based on actual experience or not. Just as a conclusion of a logical inference, in order to be reliably true must be (a1) validly inferred from (a2) a set of true premisses, so a hypothesis, in order to be scientifically acceptable, must be (b1) formally confirmed by (b2) reliable reports on observational findings.

The central problem of this essay is to establish general criteria for the formal relation of confirmation as referred to in (b1); the analysis of the concept of a reliable observation report, which belongs largely to the field of pragmatics,[1] falls outside the scope of the present study. One point, however, deserves mention here: A statement of the form of an observation report (for

[1] An account of the concept of pragmatics may be found in the publications listed in p. 22, n. 1.

example, about the position of the pointer of a certain thermograph at 3 a.m.) may be accepted or rejected in science either on the basis of direct observation, or because it is indirectly confirmed or disconfirmed by other accepted observation sentences (in the example, these might be sentences describing the curve traced by the pointer during the night), and because of this possibility of indirect confirmation, our study has a bearing also on the question of the acceptance of hypotheses which have themselves the form of observation reports.

The conception of confirmation as a relation between sentences analogous to that of logical consequence suggests yet another specification for the attempted definition of confirmation: While logical consequence has to be conceived of as a basically semantical relation between sentences, it has been possible, for certain languages, to establish criteria of logical consequence in purely syntactical terms.[1] Analogously, confirmation may be conceived of as a semantical relation between an observation report and a hypothesis; but the parallel with the consequence relation suggests that it should be possible, for certain languages, to establish purely syntactical criteria of confirmation. The subsequent considerations will indeed eventuate in a definition of confirmation based on the concept of logical consequence and other purely syntactical concepts.

The interpretation of confirmation as a logical relation between sentences involves no essential change in the central problem of the present study. In particular, all the points made in the preceding sections can readily be rephrased in accordance with this interpretation. Thus, for example, the assertion that an object a which is a swan and white confirms the hypothesis '$(x)\,(\mathrm{Swan}(x) \supset \mathrm{White}(x))$' can be expressed by saying that the observation report '$\mathrm{Swan}(a) \,.\, \mathrm{White}(a)$' confirms that hypothesis. Similarly, the equivalence condition can be reformulated as follows: If an observation report confirms a certain sentence, then it also confirms every sentence which is logically equivalent with the latter. Nicod's criterion as well as our grounds for rejecting it can be re-formulated along the same lines. We presented Nicod's concept of confirmation as referring to a relation between non-linguistic objects on one hand and sentences on the other because this approach seemed to approximate most closely Nicod's own formulations, and because it enabled us to avoid certain technicalities which are actually unnecessary in that context.

(To be concluded)

[1] *Cf.* especially the two publications by R. Carnap listed in p. 3, n. 1.

II.—THE PROBLEM OF UNREASONED BELIEFS.

By W. T. Stace.

I

We must, says a contemporary writer,[1] "distinguish carefully the task of epistemology from that of psychology. Epistemology does not regard the processes of thinking in their actual occurrence; this task is left entirely to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system; or to construct justifiable sets of operations which can be intercalated between the starting point and the issue of thought-processes; replacing the real intermediate links. Epistemology thus considers a logical substitute rather than real processes. For this logical substitute the term *rational reconstruction* has been introduced".

This view of the task of epistemology, and of its relation to psychology, is not, I think, peculiar to the author quoted. It is typical of current views, and I shall take it as being representative. There exists a body of beliefs which is in general designated human "knowledge". Some of these beliefs have been reached by processes of logical reasoning. But many of them—including some of the most fundamental and important—have not. They have been reached apparently by various "psychological processes" which do not appear to be chains of logical reasoning. Thus every normal human being "knows" that there exist minds other than his own (whatever may be meant by the word "mind"). Every normal human being "knows" that there is an external physical world and that the objects which constitute it have a continuous existence which is independent of our perceptions of them. Neither of these beliefs has been arrived at—in normal cases at anyrate—by any process of reasoning. It is the task of epistemology to discover the logical grounds for these and other human beliefs, if there are any; or if there are not, to say so.

The original aim of rational reconstruction—as one may discover by consulting Descartes, the founder of modern epistemology—was the quest for certainty. Human beliefs, just

[1] H. Reichenbach, *Experience and Prediction*, p. 5.

because they are often not reached by any logical process, appear uncertain. To validate them, to confer certainty upon them by showing that they *can* be logically justified, was the function of epistemology. No doubt this original aim has become somewhat blurred. Individual epistemologists may be directed in their enquiries by a variety of different purposes. And it is not now generally believed that factual knowledge can ever be " certain ", can ever be anything more than probable (which is itself a discovery of epistemology). Nevertheless it is reasonable to suppose that the ultimate justification of epistemological enquiries is still the validation of human knowledge, the attempt to show that at least in its broad outlines it is probably true.

In order to discover how " unreasoned beliefs "—which we are to study in this paper—ought to be defined, it is necessary to refer very briefly to the procedure which the epistemologist usually adopts in order to carry out his aim. He performs his task by showing, or attempting to show, that every item of knowledge either (*a*) is a case of immediate knowledge, or (*b*) is capable of being reached by processes of reasoning which have their starting points in cases of immediate knowledge. Thus an item of knowledge may not have been actually reached by any process of reasoning. It may have been reached by merely " psychological processes ". But if the epistemologist shows that it *can* be reached by a chain of reasoning which has its starting point in immediate knowledge, he considers that he has performed his task as regards that item of knowledge. He has justified it, validated it. This procedure assumes—for reasons which cannot be here discussed—that immediate knowledge is either certain or comes as near certainty as is possible for human minds.

Of course it is possible that a particular epistemologist may not believe in immediate knowledge. He may regard the starting points, the basic propositions, of his rational reconstruction of knowledge as mere arbitrary postulates adopted for convenience. But this has not been the usual view of epistemologists, and it is a view which, for myself, I should reject—again, I fear, for reasons which I must pass over if I am to come to the material which I want to discuss. In this paper I shall take for granted the view of epistemology outlined in the last paragraph, which has been the normal view from the time of Descartes to the present day.

By a " reasoning process " in this context I mean a process of either deduction or induction. This is not, of course, what Descartes would have meant; but I think it is in line with the

present-day procedures of epistemologists, as well as with the common usage of the word reasoning.

It is not so easy to say what is meant by "immediate knowledge". But the typical situation from which the notion is derived is that in which some fact or entity or truth is supposed to be directly presented to consciousness which thereupon, on the sole evidence of that which is thus directly presented, affirms this fact or entity or truth. Historically, various kinds of knowledge have been, at various times, considered immediate. Descartes took his own existence as immediately known by him, and made this therefore the starting point of his reconstruction of knowledge. Other examples of items of knowledge which have been, at one time or another, claimed as immediate, are innate ideas, synthetic *a priori* propositions, the axioms of mathematics, moral truths, knowledge of sense-data, introspective knowledge of states of consciousness. Even mystic "intuitions" of God have been claimed as such. In our day the extreme difficulty of knowing *what* items of knowledge are immediate, and whether they can actually be found and isolated, is fully recognised. Russell's discussion of "basic statements" attests this. But in spite of these difficulties, it is assumed that a reasoning process must have a starting point, and that this starting point must be immediately known. There must *be* immediate knowledge, even if we cannot say exactly *what* items of knowledge are immediate. Otherwise no trustworthy process of reasoning could ever get under way. And in that case it could never be shown that any valid or rational knowledge exists, or indeed that any proposition is ever true. Hence the original programme of epistemology as laid down by Descartes still stands—to show that all knowledge either (*a*) is immediate, or (*b*) is capable of being reached by processes of reasoning which start from items of immediate knowledge.

We can now define "unreasoned beliefs". An unreasoned belief is one which is neither a case of immediate knowledge nor has been reached by a process of reasoning. The fact that it *can* be so reached—as the epistemologist may afterwards show—does not, of course, take it out of the category of unreasoned beliefs. What makes it unreasoned is the fact that it has actually been reached by some psychological process which is not a process of reasoning.

It may seem strange to exclude items of immediate knowledge from the class of unreasoned beliefs, as is done by this definition. Such a piece of knowledge, it may be said, is not reached by reasoning, and is therefore unreasoned. My definition, however, is convenient for my purposes. According to the programme of

epistemology, items of immediate knowledge are taken as being self-justifying and as certain, or as near certainty as can be got. Beliefs reached by reasoning, on the other hand, are justified by the reasoning. Now what I want to discuss in this paper is those beliefs which have neither the self-justification of immediate knowledge nor the justification of reason. These beliefs, taken as they normally stand in the human mind *before* the epistemologist has reconstructed them, appear to have no justification at all. And yet we confidently hold them as parts of our knowledge. It is this situation which I want to discuss. Moreover, it will be found that all the cases of unreasoned beliefs which appear in this way puzzling to us fall within my definition—the "instinctive beliefs" which all of us hold, the vague "intuitions" on which some philosophers rely, the "propensities to believe" and the "common sense" beliefs to which other philosophers refer. They are none of them cases of immediate knowledge (or at least so I shall maintain), and yet they have not normally been reached by processes of reason. It is beliefs of this kind which I wish to discuss, and which I am here designating "unreasoned beliefs". I am not now concerned to discuss such beliefs as that "a red patch is now appearing".

II

If now we return to the quotation with which I opened this paper we find that it involves the admission that actually large parts of our "knowledge" consist of unreasoned beliefs. They are neither immediate nor have they been reached by reason. They have been reached by certain merely "psychological processes". Not only is this admission implicitly made by the author quoted. It must be made by every epistemologist who follows the normal programme of epistemology outlined above. For otherwise the *raison d'être* of his enquiry disappears. He is undertaking to show that a variety of human beliefs, taken to be true beliefs, *can* be reached in a rational way. There would be no point in this proceeding unless it were the case that in ordinary human thinking they are *not* reached in a rational way. The epistemologist is undertaking to supply a rationality which, it is assumed, is absent in ordinary thinking.

And the epistemologist must not only admit that there are non-rational psychological processes which lead men to beliefs. He must also admit that they lead to true beliefs, and even to knowledge. For this is in fact what he undertakes to show. He is undertaking to validate, to rationally reconstruct, the general body of human knowledge, to show that it *has* good logical

grounds, that it *can* be proved true. Of course it is not necessary for him to think that *every* human belief which has been reached by the non-logical thinking processes in question is true. In the course of his enquiries he may indeed show that some of them are false, or at least groundless. But unless he is prepared to believe that the general body of "knowledge" is in fact mostly a fairy tale, and that his task is merely that of arranging its parts in a self-consistent system, he must maintain that at least those beliefs which form essential parts of the general body of knowledge—our belief in a physical world and in other minds may be instanced—are true. Further he must hold—unless he is prepared to suppose that these non-logical processes have led men to truths by a mere *fluke*—he must hold that these non-logical thinking processes, though they may sometimes lead to error, yet have a definite tendency to lead to true conclusions, and have in fact led men to discover truths. He must admit that *there exist non-rational mental processes which are nevertheless ways of discovering truth.*

Now as a rule the epistemologist simply refuses to have anything to do with these non-logical processes. Having admitted, by implication, that they exist, he turns his back on them. The usual excuse for doing so is that they fall within the domain of psychology, and that no epistemological or philosophical problems are involved in them. This is the point of view adopted in the passage which I quoted at the beginning of this article. The author of that passage is seeking to "distinguish carefully the task of epistemology from that of psychology". And the passage specifically excludes non-rational psychological processes from the domain of the epistemologist and affirms that their consideration is "left entirely to the psychologist". And I think this attitude is typical of epistemologists generally.

Now this view of the matter is wholly unjustifiable. Of course the epistemologist may arbitrarily define his domain in any way he pleases. And if he chooses to define it as including only rational and logical processes of thought and as excluding non-rational mental processes which lead to truth, no one can quarrel with his choice. But even so he is wrong in assigning the whole question of such non-rational thinking processes to psychology. For they do involve problems which are philosophical, and which cannot be answered by any empirical science. And if he arbitrarily excludes these questions from epistemology, then some other place has to be found for them within the boundaries of philosophy. The philosopher cannot get rid of them by blandly handing them over to the psychologist.

For it has been admitted that these non-rational processes *lead to truth*. That is, they have a certain degree of *validity*. (I shall define, in a moment, the sense in which the word "validity" is used here. It is not the logician's sense.) The question therefore arises how, being non-rational, they can yet be valid. And this, I claim, is a philosophical, and not a psychological, question. You have a series of psychological events *a-b-c-d* . . . leading to X, which is a true conclusion. The steps *a-b-c-d* . . . are, by hypothesis, logically relevant neither to one another nor to the conclusion X. And yet not only is X true, but it is not a mere fluke that it is true. Somehow, in some way, the steps *a-b-c-d* . . . have a definite tendency to lead to truth. How is this to be explained?

Let us first of all carefully assign to psychology and to philosophy their respective interests in the situation. It is the task of the psychologist fully to *describe* the series *a-b-c-d* . . . , to tell us what happens, what actually goes on, in the mind; and also, if possible, to discover regularities (laws) in such sequences of mental events. As soon as he has done this his task is concluded. With the question of the validity of this series, of its tendency to lead to truth, he has no concern. That is the business of the philosopher. After all we make exactly the same division of labour in the case of reasoning processes. There is such a subject as the psychology of reasoning. Its task is simply to describe what happens, what is the series of events, in the mind of the man who reasons. But any questions regarding the validity of the reasoning process—for example, the problem of the nature of logic, whether it is a sphere of "eternal truths" or of merely linguistic conventions, or the problem of the nature of necessary propositions—belong to the domain of the philosopher.

Since I here speak of the validity of non-logical thought-processes I will, before going further, define in what sense the term validity is being used. It obviously is not the validity of the logician. In his language the non-logical series *a-b-c-d* . . ., although it may lead to a true conclusion, is by definition invalid. But I am choosing to use the word in another sense. I mean by a valid mental process, a process which, whether logical or not, *leads to true conclusions with a frequency which is greater than can be accounted for by chance coincidence.* This conception also allows us to speak of *degrees* of validity. If chance coincidence will explain, in a given kind of case, a true conclusion being reached once in a hundred times on the average, then a process which regularly leads to a true conclusion once in ten times will possess a certain, though rather low, degree of validity.

A process which leads to true conclusions nine out of ten times will possess a relatively higher degree of validity. To illustrate. Blind guessing has no degree of validity, where a guess is called "blind" which is based upon no grounds whatever. I might now guess, on absolutely no grounds, and knowing that I have no grounds, that the next winner of the Derby will be named "Apollo"; and by chance this might turn out to be true. But my mental process would have no validity because it would not lead me to true conclusions more often than could be explained by chance coincidence. If, however, I developed a super-normal gift of predicting the winners of horse races, without any knowledge of the horses, and it were proved after the most searching empirical tests that I could regularly predict correctly nine times out of ten, we should be forced to admit that my mental processes —whatever they might be—had a high degree of validity in the sense in which the term is here used.

Now my contention is that there do exist non-logical thought-processes which have validity—of what degree we need not at present try to determine. I am maintaining not only that there are such processes, but that the very existence of the science of epistemology involves the admission that there are. For it conceives its task to be the rational reconstruction of knowledge which has normally been obtained by non-logical procedures. One may go even further. It is evident that, if the epistemologist is right, *non-logical ways of reaching truth are the normal ways of reaching it, and the way of reaching it through reasoning, though not uncommon, is yet not the commonest way.* Thus I am not in this article proposing to discuss some kind of miraculous supernatural "faculty" possessed by mystics or other fantastic persons. I am discussing normal faculties of normal people.

I am further maintaining that these facts give rise to a philosophical, and not merely a psychological, problem—or rather to two philosophical problems. The first problem is: how is it that these thought-processes, if they are non-rational, nevertheless possess validity; and what is the nature of this validity? How can it be the case that a series of psychological events *a-b-c-d* . . . can lead to truth when, by hypothesis, these steps are logically irrelevant both to one another and to their true conclusion?

Of course it might be the case that the conclusion X has not been reached by human beings by any *process* such as is symbolised by *a-b-c-d*. . . . It might be the case that X has as it were sprung up out of nothing in the minds of men, *i.e.*, that it had no psychological or logical antecedents at all. This is a logical

possibility which should not be excluded. But even if it were a fact, it would not alter the essentials of our problem. We should then have to ask how, in such a case, can such a mental event as the sudden springing into mental existence of an idea which has no logical antecedents lead to truth?

The second of our two philosophical problems is: What varying degrees of validity have various kinds of non-logical mental processes (if there are various kinds)? In other words what *weight* ought we to attach to unreasoned beliefs either in our philosophical constructions or in our practical lives? It is certain, and I shall abundantly show, that we all do attach weight to them, both plain men in their practical living and philosophers in their systems of thought. But I think that no one has ever yet considered, or tried to discover, *what* weight, what degree of validity, we ought to attribute to them. Philosophers, scientists, and plain men alike simply attach to them whatever weights suit their fancies. Perhaps this cannot be remedied. But the subject should at least be investigated.

There is indeed one way in which these problems can be altogether side-tracked. We may allege that the general body of human knowledge is not "true" in any sense except that it hangs together, and that it is accepted by human beings. It is not, or much of it is not, true in the sense that there are objective facts corresponding to it. That there is an external world independently existing is not true in the sense that there really *is* such a world. To say that it is true merely means that all normal human beings accept it, and no doubt that it "works". Even the existence of other minds will have to be interpreted in this sort of way. (Incidentally it seems to me far more difficult to interpret in this way the existence of other minds than so to interpret the existence of a physical world. For if I suppose that my belief in other minds is merely a convention which works, shall I not have to suppose that my belief in the existence of my own mind is merely a convention which works? But this seems self-contradictory, since it must then be supposed that it was a really existing I who adopted this convention.) Human knowledge is a fairy tale invented by a being who is himself a fairy tale invented by himself! We are all equally deceived by it. If anyone is prepared to accept such a view of human knowledge, then of course he will easily understand how our main beliefs have been reached by non-rational processes. The fundamental similarities of human minds everywhere will explain our general agreement in holding them. And the task of epistemology may still be rational reconstruction in the sense that it

arranges these fundamentally fallacious beliefs in an orderly and logical system.

I admit that this is a way out. But it seems to me to be a desperate way, involving as it does a complete scepticism as to the possibility of any genuine knowledge of the world we live in, as well as a contradiction when we try to apply it to the knowledge of ourselves. I shall therefore say no more about it. I shall assume that we mean by knowledge something more than an organised system of fantasies. I shall assume that truth is reached by human beings, sometimes by logical processes, and sometimes by merely psychological non-logical processes, and that this latter fact gives rise to the problems which I have outlined.

III

I have so far stated our problems in the abstract without giving any account of the factual material from which they arise, namely the unreasoned beliefs which are actually held by men. Two examples have been briefly mentioned, belief in other minds and belief in a physical world. Before going further it would seem desirable to fill out the picture by adding some details of a descriptive kind about unreasoned beliefs generally. To do this will serve to make the problem more vivid and concrete, and also to show how far-reaching and how extremely important it is. For it will then be seen that the problem ramifies throughout the whole of human thinking, both practical and theoretical. It is not merely in regard to such questions as the existence of other minds and an external world—usually regarded as purely academic and of no interest save to philosophers—that we have unreasoned beliefs. We have them about all subjects, about the physical world, about ourselves, about each other, about morals, about our breakfasts, about our work, our play, in short about everything. The whole fabric of human thinking, from our most trivial thoughts to our most profound philosophical treatises, is shot through with them.

If this is so, it will be obvious that the factual account to be given in this paper cannot do more than scratch the surface of the subject. I do not pretend to be able to give any sort of complete account of unreasoned beliefs. My purpose is merely to indicate a few of the most important kinds in order that it may be seen how far-reaching our problem is.

Nor can I offer to classify unreasoned beliefs in any satisfactory way. For the purposes of this article I shall arrange them under three heads which, as will be seen, are not distinguished from one

another by any very sharp lines. My three heads will be: (1) Those unreasoned beliefs which receive practically universal human assent, (2) those which, though not so universal, can claim a general measure of assent by most, or at least many, civilized races, (3) those which can claim little, if any, general assent.

(1) *Unreasoned beliefs which receive practically universal assent.* These are all, so far as I can see, factual beliefs in the sense that they concern facts about the world, and not values.[1] It would be difficult to give anything like a complete list of them. Our beliefs in other minds and in an independent physical world which is common to all normal perceivers, fall under this head. Normal human beings, unfamiliarized with philosophical theories, probably also everywhere believe that physical objects, as well as merely existing when they are not perceived, also retain their sense-qualities such as colour, taste, smell, hotness and coldness. If those philosophers are right who maintain that such qualities are really subjective, then this is a case in which non-rational psychological processes of thought have led men to a false conclusion. But the fact that this can be the case does not, as we have seen, do away with our problem. For it has not been asserted that such non-rational processes always lead to true beliefs, but only that they do so more frequently than can be explained by chance coincidence.

In his *Problems of Philosophy* Russell has referred to beliefs of this kind as "instinctive beliefs". Although this label seems to imply a profoundly unsatisfactory account of their origin—that they arise out of "instincts"—it at least testifies to their unreasoned character. They are not cases of immediate knowledge. They are not given to the senses. It will not be maintained that they are *a priori* synthetic propositions or innate ideas. Yet on the other hand they have not been reached by any process of reasoning.

Sometimes they have also been called "common-sense" beliefs. The term "common sense" is no better than the term "instinctive". It serves to hide the problem. What is common sense? It is, apparently, a name of one of the non-rational ways of reaching truth. Or perhaps it is a blanket term covering a variety of different non-logical psychological processes which normally, or often, lead to truth. When we think we know something, but can give no rational account of how we know it,

[1] This distinction is made for convenience of exposition and is not meant as an admission that moral and other valuational beliefs are not also, in the end, factual.

we say "I know it by common sense", or "all common sense people know it". But no psychologist has ever even attempted to tell us what is the psychological process, or what are the psychological processes, which are covered by this term, or how they work. Much less has any philosopher ever attempted to explain how they can be valid, or what degree of validity they possess. Philosophers, however, do not hesitate to make use of them, and to rely on them, in their philosophical constructions, attaching to them whatever weight happens to suit the theories which they wish to advocate.

(2) *Unreasoned beliefs which can claim a general measure of civilized assent.* As the first class of unreasoned beliefs seemed to be all of the kind commonly called factual, so this second class seem to be all of the kind commonly called moral or valuational. The practical sphere is *par excellence* the happy hunting ground of human "intuitions". This is the reason why not only do we have avowedly intuitionist ethics, but even empiricist ethical writers such as Mill, Sidgwick, and G. E. Moore have always in the end to fall back on unreasoned moral beliefs, whether they call them intuitions or self-evident moral truths or moral axioms. And rationalist philosophers such as Kant have, of course, treated moral maxims as *a priori* synthetic propositions. Thus all schools of philosophers, rationalist and empiricist, seem to agree that moral principles have in some sense an unmediated character, that they are not the result of reasoning processes. Hence, since they cannot explain how they have been reached, they tend to treat them as cases of immediate knowledge. In my view this is merely a way of evading the problem which is being raised in this paper. My view is that all such moral principles are unreasoned beliefs in the special sense in which that term is here being used. They are not cases of immediate knowledge. Nor are they reasoned beliefs.

According to some philosophers moral ideas, being no more than expressions of emotion, are not properly speaking beliefs at all, since they have no cognitive content. I am not taking this view. Even where it is admitted that they are cognitive beliefs, the nature, origin, and justification of these beliefs is widely disputed, and this question constitutes indeed the main battle-ground of philosophical ethics. The view which I maintain is that a moral principle always has for its content some assertion about the relative advantages or disadvantages to be derived from actions; and is, therefore, in the last analysis, a factual statement which must be either true or false. Obviously I cannot adequately defend this view here, since to do so I should have

to launch out into a treatise on ethics. And since I cannot defend it adequately, it will be better not to defend it at all, but simply to state it. The question will then be: by what psychological processes have human beings arrived at beliefs about the relative advantages and disadvantages to be derived from actions, if these beliefs are neither cases of immediate knowledge nor the results of reasoning? And how can such psychological processes possess validity? In this section my purpose is merely to give some concrete examples of such unreasoned moral beliefs.

Particular moral precepts may usually be represented as deductions from more ultimate moral ideas, though some writers represent even such special obligations as the duty not to break promises as intuitions. Hence it is better not to take particular precepts as our examples of unreasoned beliefs. It is among the most ultimate moral ideas that we should look for them.

Consider the idea which may be expressed in numerous different ways, of which the following are some examples: that the soul is more noble than the body; that the "things of the mind" are more valuable than the things of the body; that spirit is "higher than" matter; that intellectual satisfactions are of an intrinsically better quality than bodily satisfactions. What exactly upon analysis these vague statements might turn out to mean is a philosophical problem of great interest and importance. But this is not the problem which I wish to discuss. That they bring together a number of beliefs about the relative advantages and disadvantages to be derived from certain tendencies of thought and action is what I should maintain. If we assume at any rate that they mean *something*—that they are not merely explosions of emotion—the point then is that, whatever they mean, they are certainly unreasoned beliefs. Of course any philosopher who wishes to put his head in the sand, to refuse to admit the existence of a problem, may put them down as cases of immediate knowledge, label them intuitions, etc., and so evade all discussion of how they are reached and how they can be valid. Philosophers who do this fail to see that they are putting a powerful weapon into the hands of those who dismiss moral ideas as mere ebullitions of emotion. For on the one hand they insist that moral ideas have cognitive significance, but on the other hand they give no rational account of how men came by them, but invoke mysterious faculties of non-natural intuition which smack of the miraculous and super-natural. The conclusion at once follows: if we cannot stomach these supernaturalisms, we shall have to deny that moral beliefs have any cognitive significance or that they are genuine beliefs at all. They are

then put down as "subjective" and as having no real validity or justification. I suggest that those philosophers who invoke non-natural intuitions are doing a poor service to the morality which they thus hope to prop up; but which in fact they are exposing to unnecessary attack. I suggest that the reasonable view is somewhat as follows: Ideas such as that the soul is nobler than the body have cognitive significance. They concern the relative advantages and disadvantages of different possible modes of life. They are thus factual beliefs about human life which theoretically could have been reached by some process of induction, *i.e.*, by observing over many generations the consequences for men of adopting these different modes of life. But they have not actually been reached in this way. They are not, however, miraculous revelations of immediate knowledge. They have been reached by some psychological process having a genuine degree of validity. The question is: what is this process, and how does it possess validity? If it is replied that the suggestion that there exist non-logical psychological processes which yet possess validity itself smacks of the miraculous, I can only urge that a full consideration of the subject will show that this is not so. As soon as it is realized that in ordinary human thinking about almost every subject under the sun true conclusions are *normally* reached by non-logical processes of thought, and that logical ways of reaching truth, though common, are nevertheless relatively unusual, it will be seen that moral ideas are no more mysterious than any others; and that, though what I am suggesting may involve us in a difficult problem, it does not involve us in any kind of superstitious non-naturalism.

I return to my purpose of giving examples of unreasoned moral beliefs. Professor G. E. Moore's *Ethics* will be found to contain a number of interesting instances. He calls them all "self-evident". I will quote three cases.

(*a*) Moore is arguing that consequences are the test of right and wrong. He points out that "there may be some persons who will hold, in the case of some particular rule or set of rules, that even if obedience to it does in some cases *not* produce the best possible consequences, yet we ought even in these cases to obey it".[1] He contends that this view is mistaken. But, he says, "there is so far as I can see absolutely no way of refuting it except by appealing to the *self-evidence*[2] of the principle that if we *knew* that the effect of a given action really would be to make

[1] *Ethics*, p. 179.

[2] This word, here and in the following two quotations, is not italicized in the original.

the world, as a whole, *worse* than it would have been if we had acted differently, it would certainly be wrong for us to do that action".[1]

(*b*) Concerning the duty to act towards the general good, and not merely towards one's own good, Moore writes: "If any person, after clearly considering the question, comes to the conclusion that he can never be under any obligation to sacrifice his own good to the general good, if they *were* to conflict . . . it is, I think, impossible to prove that he is mistaken. But it is certainly equally impossible for him to prove that he is not mistaken. And, for my part, it seems to me quite *self-evident* that he is mistaken. It seems to be quite *self-evident* that it must always be our duty to do what will produce the best effects, *upon the whole*, no matter how bad the effects upon ourselves may be".[2] In this connection Sidgwick's "axiom" of benevolence will be remembered.

(*c*) Moore is contending that some kinds of pleasure are intrinsically better than others, irrespective of the *quantities* of pleasure involved. He argues that the contrary view implies consequences which are absurd, such as that "the state of mind of a drunkard, when he is intensely pleased with breaking crockery, is just as valuable, in itself . . . as that of a man who is fully realizing all that is exquisite in the tragedy of King Lear, provided only the mere quantity of pleasure in both cases is the same". "If anybody", he goes on, "after clearly considering the issue, does come to the conclusion that no one kind of enjoyment is ever intrinsically better than another, provided only that the pleasure in both is equally intense . . . there is no way of proving that he is wrong. But it seems to me almost impossible that anybody, who really does get the question clear, should take such a view; and if anybody were to, I should think it is *self-evident* that he would be wrong".[3]

It will be noticed that in each of the three cases Moore thinks, not only that the moral principles in question *have not* been reached by reasoning, but also that they *cannot* be, in fact that they can neither be proved nor disproved. If my view is true, namely, that they are unreasoned beliefs, then the former statement is correct, but not necessarily the latter. It is part of the definition of an unreasoned belief that it has not been reached by reasoning. But the epistemological analysis of it may show either that it can be proved true, or that it can be proved false, or that it can neither be proved true nor false.

[1] *Ethics*, p. 181. [2] *Ibid.*, p. 232. [3] *Ibid.*, pp. 238 and 239.

It is indeed extremely difficult to know what Moore means to imply by the use of the word "self-evident". He may think that the three beliefs which he says are self-evident are cases of immediate knowledge. He may even think that they are *a priori* synthetic propositions. In any case I should urge that there is no good ground for accepting these claims. It seems more likely that these beliefs have been gradually arrived at by human beings, in some way as the result of thousands of years of experience of living, by some psychological process other than a process of strict reasoning. It appears to me reasonable to claim that they are neither cases of immediate knowledge nor beliefs which have been reached by reasoning.

The belief that the soul is nobler than the body and the three beliefs declared by Moore to be self-evident are cases of what are sometimes called moral "intuitions". Whitehead is never tired of urging that our various "intuitions", moral, aesthetic, religious, metaphysical, or merely common-sense, ought to be considered as "evidence" to be taken into account in constructing our philosophical view of the world, and ought to be given as much weight as the evidence of our senses.[1] He weaves such intuitions freely into his own philosophy. He may be right in his contention that they ought to be given weight since, if our view that there are non-logical ways of reaching truth is correct, there is a good chance of the "intuitions" on which Whitehead wishes to rely being in fact true. But his practice in this respect is an excellent example of my complaint that philosophers rely on unreasoned beliefs without having investigated their origin or the source of their validity, and attach to them whatever fancy weights happen to suit their views.

Since the word "intuition" has been introduced into the discussion I think it is desirable to point out that it is used in different senses, of which two should here be distinguished. Bergson, so far as I can see, uses the term as the name of a mysterious supra-rational faculty, a spiritual eye, which directly perceives certain truths incapable of being apprehended by ordinary means. This implies that intuitions, as he uses the term, are cases of immediate knowledge. But I think Whitehead has to be acquitted of this kind of miracle-mongering and superstition. I think he means by an intuition nothing more than a deep-seated conviction which human beings find in themselves, which they have not reached by reasoning, and for which, if asked, they can probably give no reason.[2] That we really do

[1] See, for instance, the preface to *Science and the Modern World.*

[2] Whitehead does not define the term, and it is useless to quote passages to prove the correctness of my interpretation. One has to gather what he means from a wide reading of his books.

have intuitions in this sense is of course one of the contentions of this paper; and I do not see how it can be denied. But it will be noted that it does *not* imply, as Bergson's use of the term does, that such intuitions are cases of immediate knowledge. It leaves that question uninvestigated and therefore open. Nevertheless the term is objectionable, question-begging, and dishonest. It is no better than "instinctive belief", "common sense" or "self-evident". It covers up and hides, and I think is intended to hide, a problem—the very problem which I am trying to bring out into the open. For although it does not strictly imply that the knowledge obtained by this means is a case of immediate knowledge, it disingenuously suggests this. It is used so as to make the reader think that the truth which is claimed as an intuition is intuited, that is, directly *seen*—just as a patch of colour is directly seen—that it is a case of immediate knowledge, and hence that there is no more difficulty in understanding its validity than there is in understanding the validity of the statement "this is red" when a red object is presented to the vision. But I maintain that this is false, and that the belief which is thus called an intuition is not a case of immediate knowledge. It is simply a deep conviction which we find in ourselves. And since it is not a reasoned belief either, the question of how it can be valid, and why it should be treated as evidence for other truths, remains obscure. And this is precisely the question I am raising.

(3) *Unreasoned beliefs which can claim little, if any, measure of general assent.* The first class of unreasoned beliefs seemed to be entirely about matters commonly called factual, the second class about matters commonly called moral or valuational. The third kind of belief can apparently be about almost any subject.

Individual men come to hold unreasoned beliefs which are more or less personal to themselves, although larger or smaller numbers of other individuals may, of course, hold the same beliefs in the same unreasoned way. Thus these beliefs may vary within very wide limits in the degree of assent which they can claim. They may be commonly held opinions, or they may be the highly peculiar and original ideas of exceptional people. They range all the way from abstruse philosophical theories to the common ideas of plain men about men and things around them. And since their subject-matter is completely heterogeneous, they form a group which might well be labelled simply "miscellaneous". All I can do is to pick out a few samples. I will begin with philosophical theories.

There are philosophers whose special doctrines seem to be

primarily reached by non-logical thought-processes, though they often seek afterwards to justify them rationally. Bradley speaks of metaphysics as "the finding of bad reasons for what we believe upon instinct".[1] It is not clear that Bradley's metaphysical beliefs were ever shared by any large section of humanity, but it can easily be understood that he held them originally "upon instinct", *i.e.*, as unreasoned beliefs, and afterwards tried to find reasons for them. Much of Whitehead's later philosophy appears to be the result of "intuitions" which are personal to himself—though they awaken appreciative responses in persons who tend to have similar intuitions, but appear meaningless or fantastic to those who do not—and which he is trying to justify by fitting them together into some sort of consistent, if vague and loosely connected, system of ideas or "vision of the world". There is perhaps a tendency to think that it is only the more "idealistic" or "tender-minded" philosophers who rely on unreasoned beliefs; and the more "toughminded" philosophers are apt to plume themselves on their own more logical processes of thought. This is a great mistake, the tough-minded philosophers being as a rule no whit superior in this respect. But their intuitions are different. To see this we have only to remember Russell's "instinctive beliefs", Moore's "self-evident" moral truths, and Moore's insistence on the rightness of "common-sense" ideas. And I am inclined to think that the most aggressively tough-minded of all current philosophies, that of the logical positivists, is ultimately founded upon an "intuition". For their theory of meaning is not—as I have tried to show elsewhere [2]—logically well-grounded. Its real basis is not evidence or argument, but simply an anti-metaphysical *bias*, that is, an instinctive [3] and non-rational rejection of certain kinds of ideas. The truth is that all philosophers, of all schools, rely heavily upon unreasoned beliefs, and that all philosophies are largely woven out of them. This is probably inevitable. And if we hold, as I do, that there are non-logical thought-processes which are (more or less) valid ways of reaching truth, it is in the end justifiable. What I think objectionable is that philosophers should rely on unreasoned beliefs without investigating their origins, without considering the very difficult

[1] *Appearance and Reality*, Preface.

[2] MIND, N.S., Vol. LIII, No. 211, pp. 215-237.

[3] I am compelled to use such words as "instinctive" and "intuition" although I have myself objected to them as question-begging and objectionable. I am, however, treating them as convenient labels and do not mean to attach to them the objectionable implication that the instinctive beliefs and intuitions to which I refer are *cases of immediate knowledge*.

problem which their existence poses, and without making the slightest attempt to assess the varying degrees of validity which they may possess. Intuitions contradict one another. Hence some of them must be false, though others may be true. At present philosophers simply hurl their intuitions at one another and try to support them by arguments which are afterthoughts. The only way to remedy this state of affairs is to raise squarely the problem of unreasoned beliefs, to make a study of how they arise and how they become valid, with a view to trying to decide which ought to be trusted and which ought not, and what *degrees* of reliance should be placed on them.

Apart from philosophers, there are in general some thinkers, writers, speakers—on all subjects—who are recognised as more intuitive than others. There are the predominantly logical minds and the predominantly intuitive minds. Most of the "flashes of insight" of the latter seem to be unreasoned beliefs. The popular idea of the "genius" is largely founded on the capacity of some men to seize upon remarkable couclusions by a kind of insight which does not work by the laborious methods of reason. Although careful reasoners are sometimes labelled geniuses, it is the intuitive type which more commonly receives the name. Their conclusions not being founded on logical antecedents seem to come from nowhere, to spring miraculously into being. In romantic or superstitious ages they are then said to be "inspired". It may be thought that their conceptions flow into their minds directly from a divine source; or that they are guided by an "over-soul". In our age some Freudian explanation in terms of the unconscious is more likely to be given. The words genius and inspiration are perhaps more fittingly applied in the æsthetic realm, to artists, than in the cognitive realm of beliefs and opinions. Yet they are applied in the latter cases too, and in any case the two realms are not sharply divided from one another. Art is partly cognitive, and science and philosophy are partly æsthetic.

An extreme example of the gift of divining truths in an apparently non-rational way is found in the cases of certain infant mathematical prodigies. There have been boys in their early teens who, when presented with mathematical problems which would ordinarily involve long and difficult calculations occupying many minutes, have been able to give the correct answers within perhaps two or three seconds. It is difficult to think that the psychological processes of such prodigies are in any ordinary sense those of rational calculation. Nor can we believe that they have immediate knowledge of remote mathematical relations.

Hence their results must be unreasoned beliefs having a high degree of validity.

Apart from infant prodigies it is well known that some mathematicians and scientists are more intuitive than others. The Indian mathematician Ramanujan is said to have reached various mathematical theorems which he was unable to prove, but which were proved true by other mathematicians after his death.

Turning now from the ideas and conclusions of well-known philosophers, mathematicians, and writers generally who receive a high degree of public attention, to those of plain average men, who receive little or none, it may be pointed out that various individuals seem to possess gifts of divining truth in various special situations. We say of such people that they have a "flair" for this or that subject. They seem not to reason things out but to "nose" them out, or "sense" them. It will be observed that the use of both these common metaphors of "nosing" and "sensing" carry with them the idea of the immediacy of sense-experience. They are thus similar to the word intuition. They at any rate imply that the truths which are nosed or sensed are not reached by a logical process. Successful business men often owe part of their success to a gift for intuitively appraising human characters and complicated economic situations. And sometimes a man will claim that he has a "hunch" that such and such is the case; and although such hunches are frequently mistaken, it may turn out that they are correct in more cases than can be explained by chance coincidence. However unreliable they may be, they seem to be better than blind guesses which, as we saw, have no validity at all.

In this section I have brought together under the one general head of unreasoned beliefs a large number of very different sorts of belief which, though each taken separately may have previously received a certain amount of notice from philosophers, are not usually classed together or thought of as having any special connection with one another. Perhaps Russell and Moore may feel a certain repugnance against the ideas which are basic to their philosophies being classed along with what they might regard as the baseless fantasies of Bradley and Whitehead. The two cases seem so very different. Again it is not usual to put moral intuitions alongside of hunches and flairs. Yet the instinctive beliefs of Russell, the common-sense beliefs of Moore, the "propensities to believe" of Hume, the metaphysical intuitions of Bradley and Whitehead, the deep moral convictions of civilised men and women, the hunches, flairs, divinings, sensings,

nosings of business men and ordinary mortals, the seemingly mysterious gifts of mathematical prodigies, the flashes of insight of certain writers—an oddly miscellaneous assemblage indeed—do all form a class, the members of which have in common the fact that they are neither cases of immediate knowledge nor beliefs which have been reached by reason. They have all been reached by non-logical thought-processes. And I think it is important that the common element in all these very dissimilar kinds of mental processes and beliefs should be recognised. And I raise as regards them all the fundamental philosophical question, how can any non-logical train of thinking *a-b-c-d* . . . lead to a true conclusion X, more frequently than can be explained by chance coincidence, when it is by hypothesis the case that none of the terms of the series are logically relevant to the conclusion or to one another?

I think it is because this problem has never been honestly faced that many mistakes have been made, many blind alleys pursued, in philosophy. Philosophers found in the human mind many valid beliefs the basis of which they were unable to understand or explain. They could not understand them because *they assumed that, in order to be valid, every belief must be either immediate or reasoned;* and these mysterious beliefs, the validity of which the philosophers could not understand, were neither immediate nor reasoned. In order to try to explain them they therefore invented mysterious and semi-miraculous powers of the mind which enabled the mind directly to *see* the truth of these beliefs. To these powers they gave names such as "intuition", "insight", "intuitive apprehension", "moral insight", "moral sense", all of which were devices for suggesting, rather furtively, that these beliefs were after all cases of immediate knowledge which would therefore be self-validating and so require no further explanation. The furtiveness, the hesitancy, ambiguity, fuzziness, the faint flavour of dishonesty, which attaches itself to such words as intuition, is due to this—that philosophers dimly perceived that the beliefs in question were *not* cases of immediate knowledge, and yet at the same time they wished to suggest that they *are* cases of immediate knowledge because they did not know how otherwise to justify them and explain their validity. They had thus to face both ways simultaneously, and they therefore generated a fog around themselves in order to conceal their strange proceedings. It was in this way that there came into philosophy the idea of a special moral sense. In this way also the whole of intuitionist ethics came into being. From the same cause it became a habit of certain philosophers to account for

those common beliefs about material objects which cannot be explained as the products of sense-experience by simply declaring them to be "*a priori*".[1] From the same source came also much semi-mystical vapouring about non-existent mental capacities, visions of truth, and the like. If ever the plain fact that we have valid knowledge which is neither immediate nor reasoned had been squarely faced and investigated, how much of all this would have been avoided! Of course, if the fact *had* been frankly admitted and faced, it would have fastened upon philosophers a new and difficult problem, the problem *how* such unreasoned beliefs can be valid. But it is better to struggle with a genuine, though difficult, problem, than to evade it by losing oneself in a fog of illusions.

IV

We have come to the point at which, the problem kaving been stated, the solution of it is due to be given. About possible approaches to a solution I shall indeed have some tentative suggestions to make. But these will be more in the hope of stimulating other philosophers to discuss the question than because I suppose myself to be able to give the answer. I believe the problem to be a profoundly difficult one. Perhaps it can only be properly investigated by co-operative discussions among philosophers over a long period. The main aim of this paper is rather to draw attention to what I consider a neglected problem than to attempt its immediate solution. I shall write down such positive ideas on the subject as I have, but I would wish to stress their purely experimental and tentative character.

There are one or two considerations which should, I think, preface any attempts at solution. The first is this. The problem is at any rate a real and important one. There may be a tendency to try to deny its reality. How can a non-logical non-rational series of mental events have a definite tendency to lead to true conclusions? One is inclined to say that it quite obviously cannot, that there must be some mistake in the way the matter has been stated, and that there cannot really be any such problem. But it is not possible to adopt this attitude without flying in the face of plain facts. It is not a philosophical speculation but a plain matter of empirical fact that there do exist such valid non-logical psychological processes. As I have pointed out, the very existence of the science of epistemology implies it and rests on it. And the factual evidence for it which is detailed in the

[1] See, for example, H. H. Price, *Perception*, pp. 168, 186, 306.

last section obviously consists of only a few samples of the masses of similar evidence which could be marshalled by a patient investigator. It is not a case of asserting the existence of strange, unusual, fantastic, semi-miraculous powers of abnormal minds—a procedure rightly subject to suspicion. These psychological processes are plainly normal. They are the processes by which ordinary men ordinarily reach a large number of their true beliefs. Hence the problem *how* such psychological processes can be valid is forced upon us, and cannot be got rid of by turning a blind eye to it.

My second preliminary remark is that we must not over-simplify matters by assuming, without enquiry, that there is only *one* kind of valid non-logical psychological process. I have in this paper classed together " instinctive beliefs " about the material world, the declarations of " common sense " about sundry matters, moral " intuitions ", metaphysical " insights ", the procedures of mathematical infant prodigies, the " flairs ", " hunches ", and " nosings " of common men in all matters. This varied collection *does* form a single class in as much as all its members have in common the fact that they are all unreasoned beliefs in the sense in which that term has here been defined. But it does not follow that they have all been reached by one and the same kind of psychological process. It is indeed obvious that, although correctly classed together for the reason given, there are very great differences between some of them and others. The class of unreasoned beliefs comprises many different kinds of belief. It is more than likely that these different kinds of belief form different sub-classes of the class " unreasoned beliefs ", and that these different sub-classes differ from one another by the fact that they are reached by different kinds of psychological process.

I have symbolised all such non-logical processes by the symbols *a-b-c-d* . . . leading to a true conclusion X. This symbolism should not be read as implying that there is only one kind of valid process. And by the same token we need not, if we find that there actually are many valid processes, abandon the symbolism. For *a-b-c-d* . . . can represent *any* such process, whether in fact the processes are many or one. All that is necessary to make the symbolism applicable is that the steps of the process or processes *a-b-c-d* . . . should be logically irrelevant to one another and to the conclusion X. And this will be true of all kinds of valid non-logical series, since this is what is meant by calling them non-logical. The symbols only represent what is common to all such series without prejudice to the differences between them.

This leads to the final prefatory remark which, it seems to me,

ought to be made. If we are ever to arrive at any complete and satisfactory solution of our problem, we ought first to have a proper psychological description and analysis of the actual psychological processes by which unreasoned beliefs have been reached. It is not likely that we can get very far with the philosophical problem until the psychological problem has been solved. We have not enough empirical material. And it may well be asked how we can discuss the validity of a process until the process itself is known. This is to a large extent true, and it should underline for us the tentative character of any conclusions we may reach. But there are some considerations on the other side. In the first place, it is quite possible to discuss the validity of reasoning processes without knowing very much about the psychology of reasoning. This is in fact what the logician does. And it may turn out that the case before us is not wholly dissimilar. In the second place, even if it is true that the psychological problem ought theoretically to be solved first, it does not follow that absolutely nothing can be done towards examining the philosophical problem now. Unfortunately the science of psychology, as at present practised, throws little or no light on the subject; and I see very little prospect of inducing psychologists to interest themselves in it. They are too busy with other things. But there is no reason why philosophers should postpone the consideration of their problem indefinitely until the psychologists have investigated the actual psychological processes concerned. Philosophers do not wait till all the facts of physics are known before they consider philosophical problems regarding the nature of matter. Nor do they wait till all the facts about morality are agreed on before they discuss philosophical ethics. The philosopher has to operate with the best empirical knowledge he can get at the time when he philosophizes; and if philosophers did not do this, there would never be any philosophy. There is therefore no reason why we should not go ahead with the examination of our problem. But the present state of affairs does mean, of course, that the philosopher will be compelled to rely for his psychological facts on himself, and on his own psychological acumen, without much help from the experts.

(To be concluded.)

III.—THE HUMAN SOUL AND THE COSMIC MIND.

By Wm. Pepperell Montague.

PART ONE.

THE HUMAN SOUL.

I. The Two Theories.

From the beginnings of European philosophy there have been two fundamentally opposed theories as to the nature of Mind and its relation to the perishable organism with which it is so intimately associated and upon which it is so pathetically dependent.

The first and most congenial of these theories we shall call Spiritualistic Dualism. It is the theory that the minds of men and perhaps of animals have a reality of their own that is not completely dependent upon the living bodies within which they grow and by which and through which their career is largely determined. That the mind can guide the body in the light of its memories of the past and its plans for the future seems obvious. And as the captain who guides his ship can leave it and survive its destruction so can the mind survive the body on which it has made the voyage of life. Those who hold this theory of dualistic spiritualism differ among themselves as to the nature of the mind, as it is in itself and apart from the body. Some would regard it as a pure spirit lacking all material properties but inhabiting space and time. Others would go still further and regard it as a transcendental entity outside or beyond the spatial and temporal world. I shall not deal with these questions for I do not want the minor disagreements among dualists to obscure the major point on which they are all agreed in their opposition to their rivals, the Naturalistic Monists.

The theory that I shall call Naturalistic Monism is the view that mind whether human or animal is an inseparable aspect of the organism and its processes, particularly those of the central nervous system. This monistic conception is not so congenial to our wishes and hopes nor even to our common-sense beliefs as is its dualistic opponent. Its great and growing strength is based upon science rather than upon naïve feeling and expectation. As science has advanced, and succeeded in explaining, or

at least in describing and predicting, the course of events in one domain after another without employing the conception of guidance by a spirit apart from physical phenomena, the conviction has grown that men and other living organisms are integral parts of nature, more complicated in their laws than inorganic bodies but not essentially different from them, or at least not differing from them in such a way as to require explanation by immaterial agencies such as spirits. What we experience in ourselves as mind is then to be conceived as the inner aspect of certain processes in our bodies, and no more separable from or independent of those processes than the shape, size or velocities of a body are separable from it. Such is the theory of Naturalistic Monism. It is, of course, what has been traditionally called materialism in that it regards Mind as an aspect or function of the material body. But those who hold the theory to-day do not like to be called materialists or even mechanists because they say that such names would convey an over-simplified and distorted impression of their view. Matter at the present time is regarded as composed not of simple little elastic spheres like tiny billiard balls but of complicated clusters of waves, and its laws are not the simple mechanical laws that sufficed to explain the behaviour of the old-fashioned atoms but highly complicated relations in which electrical and electro-magnetic factors are at least as important as those that are mechanical. Disputes about the meaning of words are so much less interesting than disputes about the nature of things that we can afford to substitute for the labels of materialism and mechanism the labels naturalism and monism or naturalistic monism as acceptable names for the current theory that is opposed to spiritualistic dualism. In making this concession, however, we must be careful to remember that the new and complicated physics of the twentieth century with its concepts of Relativity and Quantum provides no more ground than did the older physics for the purposive or teleological type of activity that appears to be characteristic of mind. There are many contemporary theologians who have been rejoicing in the death of their ancient enemies, materialism and mechanism, because they have heard that the atoms of matter are packets of waves and that the laws of matter are at least as much electrical as mechanical. These theologians are cultivating a false hope and are doomed to disillusionment for the reason that was just stated. Nature under the new dispensation is exactly as blind or purposeless as under the old. And it is as difficult to find a place for mind and its peculiar properties in the organism as conceived to-day as in the organism of preceding centuries.

II. The Peculiar Properties of Mind.

Of the many characteristics of the kind of reality which we call mind or mental, I select for summary consideration those four which seem to be most fundamental and which contrast most sharply with the kind of reality which we call matter or material. They are:

1. Privacy.
2. Duration.
3. Purposiveness.
4. Integration.

Let us consider them in turn:

1. *Privacy.*—Everything that is mental is "private" in the sense of not being externally observable. If you were to cut open a man's skull and examine with the most powerful conceivable microscope every cubic inch of his brain you could not imagine yourself observing any of his mental states. The simplest sensations or feelings and the most complex ideas or thoughts would be equally impossible to observe by anybody except the man who had them.

2. *Duration.*—Mental states are like material or physical events in that they succeed one another in time. The experiences of Monday like the physical events of Monday are followed by those of Tuesday and of Wednesday. But between the two kinds of succession there is an extraordinary contrast. In a sequence of physical events the later moments must exclude the earlier, while in a sequence of mental events the later moments can include the earlier. The material world of Monday must be over and gone before the material world of Tuesday can exist; but the *experience* of Monday's world is contained in the experience of Tuesday. Physical time has only succession, mental time has duration as well as succession. It would be nonsense to say that in a game of bridge the later hands could include the earlier, yet the player's experience of the later does obviously include in what we call memory the experience of the earlier. One configuration of the fifty-two cards in the pack gives place to another, and in the latter case no more than in the former is it possible for the past to be contained in the present. We are forced to conclude that the property of duration which is the outstanding characteristic of anything that can be called mental, simply defies explanation in ordinary materialistic or naturalistic terms.

This duration is of two kinds. There is the primary or continuous duration which is called the "specious present", and

there is the secondary or discontinuous duration which is called memory. If I make five taps on the desk, you are all conscious at the moment of the fifth tap of the four that have preceded and of the interval of time which they have occupied and which is continuous with the present moment. But if I ask you to think of a certain dinner party which you attended, last week or last year, you will be conscious of it as a part of your past, but you will not have any clear or sensory consciousness of the series of events in your life intervening between then and now. This discontinuous or episodic awareness of our past is what we mean by memory or *remembering*. Henri Bergson, who was the first philosopher to emphasize the all-important factor of " duration ", or *durée réelle* as he named it, believed that nothing of our past is ever really lost, though it may be buried so deep as to defy recollection at any later moment. Sigmund Freud and some other psychologists share this belief that memory is indestructible or immortal, at least during bodily life. I think that they are right. We drag with us through time, as a comet drags its tail, the steadily lengthening and deepening totality of traces of past experience. They constitute the private hell and heaven, the shame and the glory, that we have irrevocably built into the structure of our souls.

3. *Purposiveness.*—Anticipation of the future is the consequence and correlate of memory of the past. The possibilities which we can imagine and which we seek to actualize or to avoid are but the new combinations and rearrangements of what we have already experienced, and this third peculiar property of mind which we call *purposiveness* is but another aspect of its second property of *duration*. The capacity of the present to contain and to pursue an imagined future is at least as difficult to explain naturalistically as its other capacity to contain a remembered past. And nothing in the configuration of cerebral atoms moving from place to place will in the least account for it.

4. *Integration.*—This fourth and last of the distinctively mental properties which I select for your consideration might be called " organicity " as well as " integration ". The mind is perpetually integrating and organizing into unified systems the new elements which come to it. Not only do we assimilate into our conceptions our purposes and our characters the sensations which the environment presents, but we organize the environment itself. All the products of culture, such as laboratories, libraries and churches, are projections into the external world and incarnations in the matter of that world of the mind's own structures, its hopes and fears and questionings. Mind informs matter, and transforms

the comparative chaos of its surroundings into a cultural cosmos that is nearer to its taste. And as Life, once started, spreads and ramifies over the grateful earth from which it springs, so does that higher type of life which is Mind wage a benign and creative war upon nature, imposing upon it its own unities of meaning and value. No greater slander of Life and Mind has ever been uttered than the biologist's charge that they are adaptations to environment. The truth is just the opposite. Mind and Life adapt the environment to their needs and though they may stoop to conquer, and conform to things as they are, their primary intent is always conquest—conquest through a process of assimilation, organization and integration, by which what *is* becomes *what ought to be.*

III. The Hypothesis of a Physical Soul.

You will have gathered that I believe that Dualism has won its case, and that the opposing theory of Naturalistic Monism is quite unable to explain in its own terms the Privacy, the Duration, the Purposiveness and the Integration or Organicity that are the four distinctive characteristics of the kind of reality called Mental. But even if you are kind enough to agree that a victory has been achieved, the responsibilities of that victory are heavy and the real work has only just begun. For dualism, however true it may be, has always been scientifically sterile. The main work of science has been accomplished within the frame and by the methods of materialism or by what we agreed to call naturalistic monism. And for the purposes of science it is almost more important that a theory be fertile than that it be true. The soul and body of Plato and of Thomas Aquinas, the *res cogitans* and *res extensa* of Descartes, have given rise to insoluble problems as to the relation between the two opposed entities. And when we turn to contemporary dualists the case is the same. Bergson's *élan vital*, Hans Driesch's "Entelechies" and William MacDougall's "animistic factors" can none of them be intelligibly related to the bodies they inhabit. How can non-spatial spirit be imagined to interact with a brain spread through the space inside the skull? And worse than that, when the tried and true categories of physics are abandoned or transcended there is nothing to control or discipline our thinking. Spirits, psychoids and spooks of all kinds can be had for the asking, *ad hoc* and *ad lib*, and we are apt to find ourselves wallowing in the intellectual mud of occultism. The situation is like that which some of you may have experienced in playing a game of poker

"just for fun". Without the sobering control of a monetary stake, however small, the bets become irresponsible and fantastic, and the whole game degenerates into childish nonsense.

Now when we find ourselves in this *impasse* in which we have to choose between the fruitful falsity of monism and the sterile truth of dualism, is there anything that can be done? I believe that there is, and I request your attention to a certain hypothesis which I have very much at heart and which I think will fill the bill and really meet the needs of our problem. It is the hypothesis of a *physical soul*, something that possesses all the four peculiar properties that we have seen to be distinctive of mind, but which at the same time is capable of being described in terms of physics and described in such a way that its own nature and its relation to the body can be clearly understood.

Let us begin by noting that Energy, which is the basic category of physics, and which can be roughly defined as the quantity of motion in a system, is of two fundamentally distinct types, kinetic and potential. Kinetic energy is actual motion whose quantity is measured by the product of the amount of mass that is moving and the square of its velocity; mv^2. Kinetic energy is visible or externally observable. Potential energy is not visible or externally observable. Its amount is measured by the product of the force (ma) and the distance through which the force extends; $ma \times s$. The quantity of potential motion $ma \times s$ is algebraically equal to the quantity of actual motion, mv^2, which it is capable of producing. Now to whatever extent a motion is changed in direction its energy passes from the visible or kinetic form into the invisible or potential form. If the question is asked "What *is* this potential energy *in itself?*" a natural answer would seem to be that it is just the motion of the small particles composing the mass which had the visible motion. These motions are too small to see and in that sense their energy is invisible. This answer, however, will not do, for the simple reason that whenever the particles, be they electrons, atoms or molecules, have their motions changed in direction—their small kinetic energies will be partly or wholly transformed into small potential energies and the problem of the nature of potential energy will be back again on our hands. The answer given by the physicist is a different one. Potential energy is just the result of past motion and the possibility of future motion. In short, potential energy is *nothing but* potential; it has in itself no mysterious nature or actuality. I move a body upward or further away from the earth, and the kinetic energy of that motion is changed into the kind of potential energy that we call energy

of position. Or if I wind the stem of my watch, this motion of my winding will be changed into the kind of potential energy that we call elastic stress in the mainspring, which means only that there is the possibility that the equivalent amount of motion will slowly trickle back into the realm of external observation in the form of what to me will be the useful revolution that the hands of my watch make around its face or dial during the next twenty-four hours. But whether the potential energy is gravitational, elastic, electro-static or magnetic, it is in any case nothing but the result of a motion that is past and the possibility of a motion that is future. If I ask the physicist whether there is not something a little queer in the fact that a good concrete thing like a motion can pass into something that in itself is nothing and then emerge from that nonentity with its quantitative identity unscathed by its period of annihilation, he will brush the question aside as inconsequential or meaningless. It's merely the way things are. A state of affairs in which motion is observed will be succeeded by a second state of affairs in which motion is not observed. And that in turn will be succeeded by a third situation in which motion is again observed. The second state is the potentiality of the third state. And that is all that potential energy means. He might be good-natured enough to add that even if there were some hidden characteristic in the state called " potential ", it wouldn't interest him, for the reason that he as a physcist is interested only in what is externally observable.

At this point I will ask you to consider a kind of episode which could arise in the case of two friends going out together on a hike. One of the friends is a follower of the strenuous life. He likes walking for its own sake, and he views the rests that they both must take as regrettable interludes, mere potentialities of further walking. The second friend is not at all of this type. He is lazy and contemplative, and he views the actual walking as regrettable interludes, mere potentialities of the delightful rests in which one can enjoy the view. The first and more energetic friend seizes the right to christen the alternating phases of the hike, and he names the resting periods as just potentialities of walking. The other fellow, were he given the opportunity, would do the naming in reverse and characterize the walking as just the potentiality of resting.

Now to return from fable to fact—when a sensory stimulus passes from a sensory end-organ such as a retina, a basilar membrane or a patch of skin, it proceeds as a current of kinetic energy, theoretically if not actually open to external observation, to the

brain, and there at the synapse the current is temporarily dammed up in the form of a potential and not even theoretically externally observable energy until it acquires sufficient intensity to spark across the gap between the neurons. Now suppose that at the moment when the energy of the current passes from the externally observable phase into the purely internal or potential phase, the man who owns the brain exclaims, "Ouch! I've got a sensation!" I think we should all feel it was a pretty good bet that what from the external standpoint was a mere result of the past afferent current and a mere potentiality of the future efferent current of motor reaction, was *in itself* the sensation. In other words when the externally observable becomes *nothing* the internally observable becomes *something*, namely a mental state. Now of course a pure physiologist would be quite indifferent to these mental states even if he were kind enough to admit their existence. For him as for the physicist it is only the stimuli and the reactions that would be of interest for they alone can be observed from without. But the man himself who undergoes these currents of neural energy could be pardoned if he felt it a little odd to regard his sensations and his entire conscious being as nothing but the possibilities of the bodily reactions to which they gave rise.

So far I have been speaking more or less in allegories and fables seeking to placate your possible opposition based perhaps on your pre-existing assumptions. I want now to abandon this policy of appeasement and proceed to a definite offensive. And to that end I ask you in the colloquialism of the moment to "give me a break"—consent to view my hypothesis, if not with actual favour, at least with an open mind and a willingness to contemplate sympathetically what I think are its implications. If you will agree to that, I can save time by stating those implications categorically or even dogmatically and without attempting much in the way of a justification of each point.

Let me begin by confessing that what I said about the transformation at the synapse of one form of energy into the other was an over-simplification. Currents of kinetic energy *are* transformed in whole or in part into states of potential energy which are experienced as mental. But there is no one synapsis in which all such transformations occur. I am not attempting to revive Descartes' pineal gland as a cerebral centre at which the psychophysical miracles take place. As we shall see in a moment, it is the complex field of force extending more or less throughout the brain, in which and by aid of which the kinetic energies of the stimuli are changed into the potential energies that for us are actual sensations.

I think we may assume from what we know of ourselves that the intensive energy that constitutes a conscious state is not entirely expended in the motor reaction resulting from it. A specific trace appears to remain in the form of a memory image, and these traces successively superposed in an intensive hierarchy constitute the memory system. In the various forms of materialism or naturalistic monism that have been and are still being advocated, the memory system or mind is regarded as an inner aspect of the *kinetic* energies of the neural currents flowing through the brain and nervous system. The defenders of this view have recently been making quite a point of speaking not of "a mind" and "a body" but of a "body-mind", seeking to emphasize by the hyphen in this compound word the monistic identity or inseparableness of the mental and bodily components. We have pointed out the reasons why such a conception is untenable. The principal reason was that the successive moments of a sequence of motions are mutually exclusive so that there is no possibility of the later moments including the earlier and so providing for that *duration of the past in the present* which is the primary characteristic of everything that is mental.

The theory that I am proposing does something new; it identifies the mental not with the kinetic but with the *potential* energies in the brain. By so doing we can clearly see that the four peculiar properties of the mental, as revealed by introspection and seized upon by the spiritualist as justifying his dualism, can each be found to characterize a field of potential energy. Let us take each of them in turn:

1. *Privacy.*—Potential energy is the only thing definable in physical terms that is private in the sense of not being open to external observation. The moment when present motion disappears from view and becomes a mere potentiality of future motion, is the moment when the private or internally observable actuality of sensation makes its appearance.

2. *Duration.*—Even such a simple field of potential energies as can be carried by a coiled spring can have impressed upon it a series of kinetic energies, and then can retain superposed in an intensive hierarchy the successive members of the series. The temporal order is preserved and the spring when released can regurgitate in a new series of motions of recoil the series which it had received and retained. But during the period of retention the series exhibits not succession but duration which is the presence of the past in the present.

3. *Purposiveness.*—We have already seen that the imaginative anticipation of the future is, so to speak, the obverse of a memory

of the past. And any field of potential energy which is the result of past motions is by the same token the anticipative possibility of future motions. And while at first hearing it might seem fanciful to attribute purposiveness or teleology to a gravitational or electric field of force, it is not fanciful to realize that such a field determines the particles that come within its scope to a predestined end. If the mind is such a field, though immeasurably more complex, then the series of actions determined by it would tend towards the ends that were in conformity with its structure.

4. *Integration.*—A field of potential energy or force exhibits something of the same power of integration or organicity which we found to be characteristic of mind. Kinetic energies are passed on by " conduction ". One part of space gives up the motion contained in it to other adjoining spaces. But force or potential energy can be propagated by " induction ". The structure of an electric or magnetic field without itself being wasted can induce a replica of itself upon other material. The iron filings sprinkled upon a paper beneath which is a magnet, are integrated or organized in conformity with the Faraday lines of force constituting the structure of the magnetic field. And if the mind is a field of force, we can see how each new sensation that it acquires is organized into its structure, and gets stamped upon it by a kind of induction a meaning that is in conformity with the mind as a whole.

The mind as thus conceived is *an organism within an organism.* The living body takes its energies as contained in the food which it ingests and which by an activity akin to mind it builds up or anabolizes into its tissues. The brain, however, takes its most characteristic energies " neat " as pure motions of neural stimuli undiluted by matter. These energies are transformed from the kinetic into the potential type which are sensations, traces of which are integrated into the enduring fabric of memory.

The mind as constituted in this way is not an adjective of the body any more than a plant is an adjective of the soil from which and within which it grows. It is no epiphenomenal concomitant of bodily processes. It is a substantive entity existing in its own right, a veritable *soul,* which may even outlast the perishable organism on which and through which it acts and upon which it so largely depends. The theory that I offer is then a real dualism ; but please note that it is dualism with a difference. It is not, as are the traditional dualisms, open to the charge of being methodologically sterile and irresponsible. No less than the naturalistic monism which is its rival, it interprets the mind in terms of those same physical categories which are essential to genuine progress in science.

PART TWO.

THE COSMIC MIND.

WHEN we turn from a consideration of the nature of the human soul and its relation to the body to consider the hypothesis of a cosmic mind, we are impressed with the meagreness of evidence available to support such a hypothesis. If we lay aside the rose-coloured glasses of religious tradition and look at the cosmos coldly and with the naked eye of reason or at least of a merely secular imagination, we see a vast expanse of stars some of which like our own star, the sun, may have planets on which life as we know it could exist. Every hundred billion or so of these stars appears to be organized into what we call a galaxy, each of which rotates around its centre. The galaxies or star systems, extend through a distance of at least half a billion light years (that is for some sextillions of miles) in every direction as so far discovered. There are certainly more beyond and their number may be infinite. Nowhere among these sprawling galaxies do we discern, nor have we the slightest reason to infer, any structure however vast that resembles the living organisms that we know on our planet to be the vehicles of mind. Is it then more than an arbitrary fancy to suppose that the system as a whole, whether finite or infinite, could be the embodiment of a cosmic mind? I think the answer depends upon whether or not we accept as sound the theory propounded in the first part of my lecture. If mental states are identical with forms of potential energy then the extent to which some sort of mental reality is present in the universe will be the extent to which potential energy is present—and that is everywhere. But the thought will immediately occur that the merely mental is not mind. Protoplasm is the only agency we know which acts as a trap for energy, and by means of which the traces of the kinetic currents flowing through it are preserved, accumulated and organized. In non-living systems there is mind-stuff but not mind. Energy is dissipated almost as fast as it is received and leaves no traces of sufficient strength to propagate themselves and to determine appreciably the behaviour of the system in which they may be stored. On the other hand, however, it would seem to be highly improbable that the particular combination of carbon, hydrogen, oxygen and nitrogen, which constitutes our protoplasm and which can only exist under highly restricted conditions of gravity and temperature, should be the only combination of atoms to serve as a vehicle for those accumulations of potential energy that mean life and

mind. But whether or not there are other types of finite minds with other material embodiments than those we know on this planet, it is difficult not to believe that the cosmos as a whole possesses a completer unity than any of its parts, and thus constitutes an integrated field of energy having an organicity like that of mind and a memory from which nothing of the past could be wholly lost.

There are, however, two roads " through nature to God ", and I propose now that we turn to the second of these roads and consider not the structure of the cosmos in space but its history in time in order to see if we can there discover anything indicative of the work of mind. Does the universe show any evidence in its behaviour of being created and guided by a mind that is both omnipotent and benign? To this time-honoured theological question the answer must be a flat negative. Nature is red in tooth and claw, and life is so constituted that each creature can preserve its existence only by devouring other creatures. The will to live is, as Schopenhauer said, a hungry will and feeds perpetually upon itself. If there is a God, He is either not omnipotent or not good, in any sense of the word " good " that the human conscience can sanction. When transfixed on the horns of this ancient dilemma which is called " The Problem of Evil ", the theologian has usually preferred to save the omnipotence of God at the expense of His goodness. We are told that God works in a mysterious way His wonders to perform, and that His ways are not our ways. For a being with a conscience anything like ours such a God would be regarded as diabolical rather than divine if, *with omnipotence to draw upon*, he had made a world in which 99 per cent. of his creatures were to suffer agony and defeat in order that one per cent. might thrive. And where does a vainglorious anthropomorphism reach such a climax of impudence as when the defenders of this type of theology seek to explain and justify the misery inherent in animal life by declaring it to be the proper result of human sin? As though anything that was done by so recent an inhabitant of this planet as man could justify the suffering that took place in the ages that preceded man's appearance! Might does not make right even when it is divine might. And power politics is as ugly a thing in heaven as it is on earth. To attribute to a God of Love the creation of the world of things as they are is blasphemy.

Suppose then, that we purge our religion of the immoral notion of divine omnipotence and, freed from that embarrasment, look again at the course of nature. At once the picture changes and brightens. The conflict, tragedy and waste are still there, but

pervading them all we find unmistakable signs of an upward or evolutionary trend. As life advances it deepens and sweetens and by the growth of sympathy becomes broader and less discordant. Through many a set-back and defeat this Increasing Purpose runs. No aggregate of blind forces however complicated could by a miracle of chance have given us even the life that we have, imperfect though it be. Everywhere throughout organic nature there is the sign of something like mind dimly but assuredly at work. And when we turn from biology to physics and from the little oasis of organic life to the vast desert of the inorganic by which all protoplasm is encompassed, we find what is I think an unmistakable echo of one of the four primary characteristics of mental reality, the one which we called "integration" or "organicity".

Science does to be sure record everywhere the opposite of integrations. It finds disintegrations, katabolisms and break-downs throughout nature. Molecules disintegrate into atoms, and atoms by radioactivity disintegrate into sub-atomic corpuscles such as electrons, protons and neutrons. Systems tend to pass from organization and differentiation into disorganization and uniformity. Energy tends to be dissipated and lowered in grade from molar or mechanical motion to molecular motion or heat, and finally to waves of radiant energy or light which is the most "dissipated" and least organized of all. This omnipresent trend used to be called the Law of the Dissipation of Energy, and also, under another aspect, the Second Law of Thermodynamics. It is now frequently called the Increase of Entropy, where the positive-sounding word entropy means the amount of uniformity, extensity, scatteredness and disorganization, which so far as significance and utility is concerned is very negative indeed. To congratulate a sick friend on his increase of entropy would be to congratulate him on his increase of decay and dissolution! But is it not true that this sad trend in nature must have been preceded by an opposite trend which we can call anti-entropy?

Before there can be spending there must have been saving, before katabolism there must have been anabolism. In short there cannot be death unless there has first been life. Somehow or other the corpuscles must have been organized into atoms and the atoms into molecular and super-molecular systems as a prelude to the series of break-downs which were characterized as the dissipation of energy. I think we may say that most of the discoveries of science up to the present have pertained to the break-downs and to the processes incidental to them. It is true

that some syntheses of the less organized into the more organized have been observed and even created in our laboratories, but nothing that approaches the broad and deep integrations of nature has been accomplished by man. This is to be expected, for it is easier to destroy than to build up and far easier to understand the production of death than the production of life. Aristotle reminded us that what is last in the order of Knowledge is first in the order of Being; and there may come a time when a second volume of science is written in which not entropy but the anti-entropy which preceded it will be the principal theme. And yet if that second and greater volume is ever written it may be written in a different language for the good and curious reason that the powers that make for integration and organicity are, as we have already seen, characteristic of what is essentially internal or mental, and as such not open directly to external observation any more than are the minds of other people.

All this suggests a hypothesis which I do not think is too far-fetched or fanciful. It is the hypothesis that the anti-entropic power that must exist in nature as the cause of the organizations with which nature is filled is a mental power akin to what we find in ourselves. To call such a factor the Will of God would then be no empty metaphor but the very truth. For it is in Matthew Arnold's great phrase "a power not ourselves that makes for righteousness". That it is not an omnipotent power is all too sadly obvious. Everywhere there is the conflict of wills and tendencies that makes for disorganization and pain. But among these varied tendencies, and leavening the chaos which they constitute, is the tendency to higher organization and to a higher harmony.

But now in conclusion there is a final question that must be asked. If the cosmic mind or God is not omnipotent, does that mean that the chaos of nature is something outside God and menacing Him with a more or less unpredictable fate? The brave Zoroastrians and after them William James, John Stuart Mill and many others who have preferred to save God's goodness at the expense of His traditional omnipotence have seemed to think so. With all respect to the great leaders of that school of thought, I cannot believe that in this point they are right. If there is a cosmic mind, or God, then everything moves and has its being within that Mind. I prefer to think of the chaotic tendencies of Nature as what Boehme or Meister Eckhart characterized as "that in God which is not God". Cosmic evolution could then be regarded as the work of organizing the independent centres of activity in the divine mind that constitute its contents taken

distributively and as a plurality, and informing them more and more with the harmony characteristic of that mind when considered in its collective unity.

When viewed in this way and interpreted in the language of the noblest of the legends of ancient Greece, the Will of God would not be the analogue of any omnipotent Zeus but rather of the Prometheus, the Divine Rebel, whose heart was directed singly to the good and who waged an unyielding war against the Tyrant's claim that Right be subordinated to Might.

If such a Promethean Spirit is indeed a reality he should be thought of not as a King of kings but as a Comrade of comrades, needing our aid as we need his in that unending pursuit of the ideal which for God no less than for Man makes up the meaning of existence.

IV.— DISCUSSIONS.

POSITIVISM.

In an attractively presented paper ("Positivism", MIND, N.S., Vol. LIII, No. 211, 1944) Prof. W. T. Stace puts forward the following contentions: (*a*) Positivism entails a principle of significance which he calls the "Principle of Observable Kinds"; (*b*) this Principle is neither an arbitrary stipulation nor an inductive generalisation based upon a fair sample of facts selected without bias; (*c*) nor is it a development of Empiricism; (*d*) it is in fact false; and, as an appendix, (*e*) the correct principle of significance would admit as significant many statements that positivists reject as not being so. It will be convenient to discuss these points in the order (*c*), (*b*), (*d*), (*a*), (*e*).

(*c*) In §§ 11, 12 Prof. Stace wishes to show that Positivism is not a legitimate development of Empiricism, *i.e.* that the arguments that support Empiricism do not support Positivism or rather the Principle of Observable Kinds. He does this not by stating these arguments and showing that they lend support in the one case and not in the other, but by pointing out what he holds is an essential difference in the fundamental assertions of the two positions.

If Empiricism means that all *knowledge* is based upon experience, it concerns the distinction between statements that can be known to be true and those about which we can never find out whether they are true or false. But Positivism is concerned with criteria of significance of statements. Thus Empiricism is concerned with knowledge and Positivism with significance; hence the two are essentially different and there can be no way of deriving one from the other.

If Empiricism means that all *ideas* are based upon experience, we have a criterion of meaning of *words*. But Positivism is concerned with criteria of significance of *statements*. Hence his conclusion is as before.

Now Prof. Stace thinks that positivists rest their case upon the second of these interpretations. But they surely have the first in mind also (in fact I think they have in mind the first solely), for they would interpret it as a criterion of significance, on the grounds that statements about which we can never tell whether they are true or false are thereby without significance.

However this may be, it is surely in the spirit of Empiricism to include within it, though we cannot deduce from its historical forms, this application of significance to statements as well as to words. It is difficult to see why Prof. Stace denies this merely because of the sharp distinction, which he makes for another purpose, between the meaning of words and the significance of statements (pp. 215-6). These considerations about Empiricism do not, of course, constitute Prof. Stace's basic argument; and therefore they can no more

provide a refutation of his general view than he can establish it by means of them.

(*b*) Prof. Stace considers that the Principle of Observable Kinds, and therefore Positivism, cannot properly be obtained by inductive generalisation (§ 10), because positivists that have regarded it as such appear to have based it upon a non-random sample of statements, selected unfairly in two different ways: they appear to have based it on scientific statements only, which ignores philosophical kinds and others; and even then on only certain classes of scientific statements. I do not know what view positivists have taken about the status of their principles or whether they themselves are clear about it in their own minds; but it seems reasonable to regard Positivism as an inductive generalisation so far as it is based on scientific statements, and as an arbitrary definition of "significance" so far as non-scientific statements are concerned. This procedure would be unobjectionable if Positivism were trying to state the fundamental principles of science. If, however, it is trying to confute speculative philosophy, it would be doing something like trying to demonstrate to children that there are no fairies, which cannot be done. Positivism, taken beyond its scientific application, is simply an assertion of attitude—"I do not believe in the transcendent." With regard to Prof. Stace's other charge, that Positivism is based upon an unfair sample of *scientific* statements, this would be true, if it were correctly represented by the Principle of Observable Kinds; that it is not true will be urged in (*d*) below.

(*d*) Does the Principle of Observable Kinds, and does Positivism, cover such scientific concepts as gravitation? Consider the statement, "The attraction between equal masses is inversely proportional to the square of the distance between them". For Positivism, it has significance because in conjunction with statements of a directly verifiable kind it leads deductively to a directly verifiable statement. The same holds good for statements about electrons without giving them the phenomenalistic interpretation that Prof. Stace supposes positivists to have adopted (p. 227). The Principle of Observable Kinds, on the other hand, would not allow that such statements were significant. Hence, it would seem, positivists would agree with Prof. Stace that this principle was false. A positivist might hold, however, that it was false if applied universally, but that it was true of some classes of statements. This point will be taken up in the independent discussion given below. With reference to the previous paragraph, it is clear that, though the principle cannot be an inductive conclusion based upon a fair sample of scientific statements, Positivism itself may quite well be so based.

(*a*) It follows from (*d*) that, if the Principle of Observable Kinds fails to apply to a certain class of scientific statements to which Positivism does apply, then the Principle is not entailed by Positivism. If this is correct, there must be a mistake in the argument by which Prof. Stace tried to prove the entailment. He develops an argument

(§ 7) in which P is a statement whose significance is to be tested and Q a statement that verifies it, by distinguishing the case where P leads to Q by a deductive inference from that where P leads to Q by a causal inference; and he is able to show the entailment quite clearly for the latter. His argument with regard to the former *might* hold if P was a statement such as "Water boils at 100° C"; but he seems to have overlooked such statements as the Newtonian one about attraction, and here the entailment plainly does not hold. He would doubtless reply with his *ad hominem* argument (p. 225): "It is notorious that positivists take the view that logical deduction is linguistic transformation. Therefore it does not lie in their mouth to object that the facts stated in P might be of a wholly different kind from the facts stated in Q." If Prof. Stace were right about this, he would have put his finger on a fundamental contradiction in Positivism. But, though the argument might hold against all statements other than those illustrated by attraction, against this kind I cannot see its force. P need not state a fact at all. There is nothing in the language thesis, so far as I can see, to suggest that, if "Q" asserts Q, there must exist a P to be asserted by "P".

(*e*) Prof. Stace's own criterion of significance is as follows: (i) ideas must conform to the Principle of Empiricism; and (ii) words standing for them must be assembled in sentences according to rules of logical syntax (§ 13). This he states would allow statements about unknowable physical objects, and the like, to have significance—"There exists a physical object which it is logically impossible to observe, having intrinsic qualities which we can never perceive, and which is causally related to our sense-data" (p. 237), but he has not noticed that an unknowable physical object does not pass the first part of his own criterion. Positivists would probably not accept the criterion, because a narrow Empiricism that required each separate word to have meaning would not allow significance to statements such as our Newtonian example.

A comment should be added on Prof. Stace's definition of the Positivist Principle (p. 215): the word "factual" should, I think, be deleted. This is important. His definition, as it stands, is in keeping with his omitting to discuss statements like the Newtonian one.

In order to clarify the meaning and application of the Positivist Principle, it is desirable to distinguish three classes of statements: (1) those statements that can be understood prior to their verification; (2) those that cannot; and (3) those about which this feature is debatable. This is not a precise or fundamental description, but the examples that follow will show what classes of statements are meant.

(1) The first may be illustrated by the statement that there are mountains on the other side of the moon. This may be verified not directly but in principle; we know what the experience would be like of finding mountains on the other side of the moon and what the

experience would be like of finding no mountains there; we know what sort of experience is relevant to the truth or falsity of the statement. The Principle of Verifiability involved here is equivalent, it seems to me, to the Principle of Observable Kinds. We are not confronted here with the paradox that the meaning of a statement about the present or the past is given by statements about the future; for statements about the future are merely relevant to, or constitute evidence for, the existence of meaning but do not contain, define, determine, prescribe, or confer meaning. Thus the principle, "The meaning of a statement is determined by its verification", is false for statements of this kind if it is interpreted by "The meaning of a statement is the method of its verification".

(2) The next type of statement may be illustrated by "Jehovah is angry". We cannot verify it in principle; we do not know what the experience would be like of finding it true or false; we do not know what sort of experience would be relevant to its truth or falsity. The principle here used is *not* equivalent to the Principle of Observable Kinds. In what sense, then, can it be verified? As a result of questioning someone who asserts that Jehovah is angry, we may find that there is simply one test that he has in mind, namely that it is thundering. Hence, according to this principle "Jehovah is angry" is equivalent to "It is thundering". Here the meaning of the statement *is* determined by its verification or *is* the method of its verification. This has a parallel in the work of some archæologists: suppose we dig and find a bronze axe, may we say that it belongs to the Bronze Age? Dr. Daniel's comment is that this "is almost saying no more than that it is made of bronze, which is perilously near saying nothing at all" (G. E. Daniel, *The Three Ages: An Essay on Archæological Method*, Cambridge, 1943, p. 40); that is to say, according to one usage, if an excavation is classified as being of the Bronze Age, this is verified simply by finding bronze objects.

(3) The third kind of statement is illustrated by the Newtonian example already given. It is not susceptible to the kind of treatment just described. It must be dealt with according to the Positivist Principle, that, from a conjunction of it with statements that are significant according to the Principle of Verifiability used in (1) or that satisfy the Principle of Observable Kinds, we can *deduce* a statement that is significant by this same standard. Here, again, the meaning of the statement is determined by its verification, though whether or not this has the sense of being given by the method of its verification is a matter requiring further discussion.

Positivists write as if one and the same Principle of Verifiability covered all three of these clases of statements. Whether this is the intention of any of them I do not know, but it seems to me plain that it ought not to be.

For brevity let us refer to the three kinds of statements as the screened-mountain, the Jehovah, and the gravitation cases.

The first point to notice is that, since the Principle of Observable

Kinds applies only to the screened-mountain case, this Principle is *a tautology;* for all statements to which it applies are members of the class of observable kinds. In other words we cannot use this Principle to distinguish significant from non-significant statements, for we have to know something about a statement before we can apply the Principle to it, namely that it falls within the class of observable kinds. Hence for this class of statements we have, not a principle, but a predicate-label, either "observable kind" or "verifiable". Prof. Stace's formulation of the Principle of Observable Kinds is clearly of value in making it possible to bring out this point and in leading to the distinction between the screened-mountain case and the others.

The Positivist Principle—provided that in Prof. Stace's formulation (p. 215) "factual proposition" is replaced by "general scientific hypothesis"—seems to be quite definitely the correct criterion of significance with the gravitation case. Is the Jehovah case in the same position? With it, additional premisses are not required to lead deductively to a directly verifiable statement. One might therefore suppose that it was related to the gravitation case as immediate inference is to mediate—not a sufficiently important difference to warrant separate consideration of the two cases. But with the Jehovah case verification performs two functions: it acts as a *test* of significance, and it provides the *meaning* of the statement; and the question arises whether the same is true of the gravitation case. Here the verification certainly reveals the *existence* of significance; but opinions may differ about whether or not it also shows what that significance is. Some positivists seem to hold that it does, and their interpretation of the gravitation case may conveniently be thought of as phenomenalist. On this view, there would be no essential difference between this kind of statement and the Jehovah kind.

Arguments can be found, however, to show that the gravitation statement cannot rightly be regarded as of the same type as the Jehovah, or interpreted phenomenalistically, *i.e.* that its verification confirms only the existence of significance and does not prescribe what that significance is. Two are here offered. (i) Verification of the gravitation case consists in general in statements about the future, and no doubt its actual truth has an indefinite reference to the future. But, after the occasion of its first verification, it may be said to have a meaning, because, though its verification may continue, its verifiability has been established. Thus it seems preferable to say not that its meaning lies (phenomenalistically) in future verification but that the existence of its meaning is guaranteed by its having proved verifiable or amenable to empirical testing. This treatment would require the gravitation and Jehovah cases to be placed in different categories. (ii) The positivist treatment of the Jehovah statement makes it *synonymous* with its verification; we have two sets of words having the same use; and there is no value in using the one instead of the other—in fact the Jehovah statement

is apt to be misleading in a way its verification is not. On the other hand the gravitation case is not synonymous with any of its verifications. Here in fact the difference is that between the types of verification appropriate to categorical and hypothetical statements.

The positivist may ask for a criterion of meaning in this sense in which it is independent of its verifications though not independent of being verifiable. I suggest that "meaning" means *the possibility of obtaining verifications.*

This part of the discussion may be expressed by saying that there are different levels of verification: verification of a hypothetical statement would consist in deriving from it a categorical statement which would have a verification in a different sense, *i.e.* which would be an instance of an Observable Kind (where this phrase refers not to terms but to statements). Less precisely, verification of a hypothetical would consist in relating it to Observable Kinds.

Thus, it seems to me that positivists have confused in one formula two criteria of significance (if we disregard the screened-mountain kind): one applicable to scientific hypotheses and the other to speculative philosophy. This amounts to an arbitrary decree that speculation must conform to the principles of science. However one may sympathize with this aim, it cannot be proved. The speculative philosopher will always maintain that the Jehovah statement has a significance over and above what is contained in its verification. The issue may be amplified as follows.

All that the positivist is entitled to say is that speculative statements cannot be brought into relation with any sense-experience. To this the speculative thinker's reply is one of agreement, for he never meant them to be relatable to sense-experience.

The speculative philosopher would maintain, however, that his statements, though not relatable to sense-experience, were relatable to experience in a wider sense.

The positivist may then deny that this Experience affords evidence of anything objective, *i.e.* deny that it is an experience *of* anything. But the speculative philosopher would refuse to accept this.

The positivist would have no grounds for his denial; he can give only an arbitrary definition of what avenues of experience lead to the objective. On the other hand, the speculative logician cannot prove his contention either.

This is a complete impasse, which philosophy cannot overcome.

The following way of dealing with this stale-mate may be suggested. The speculative position cannot be disproved; all that can be said is that one should no more attempt to disprove it than attempt to disprove the existence of fairies where children are concerned. Children have experiences that are described in terms of fairies; a good fairy will fulfil all a child's hopes—to-morrow; speculative philosophers have experiences that are described in terms of a harmonious Absolute in which all conflict is overcome. With the power both of the fairy and the Absolute, "it must be, therefore it is."

J. O. WISDOM

A NOTE ON THE "PARADOX OF ANALYSIS".

PROF. MAX BLACK has recently undertaken [1] to show that there is no paradox of analysis even if analysis in Moore's sense is about concepts. One of the consequences of this paradox, stated by Prof. C. H. Langford [2], is that a sentence like :

(1) The concept *being a brother* is identical with the concept *being a male sibling*.

expresses the same proposition as :

(2) The concept *being a brother* is identical with the concept *being a brother*.

Prof. Moore believes that (1) is true but thinks it does not express the same proposition as (2). Prof. Black tries to show conclusively that (1) does not express the same proposition as (2). In this note I want to show that the argument which Black offers for this view is wrong. It should be pointed out that I confine myself here to Black's argument, and that I am not committing myself to any view on the synonymity of (1) and (2).

Black argues that (1) may be regarded as expressing a relation B between the concepts brother, male, and sibling, the latter three terms abbreviated as "b", "m", and "s" respectively. Thus (1) expresses the same proposition as :

(3) $$B(b, m, s)$$

and (2) expresses the same proposition as :

(4) $$b = b$$

where the double-bar symbolizes identity between concepts.

Now, so far as I can see, Prof. Black concludes that (3) does not express the same proposition as (4) simply because the relation B is different from the relation of identity. It follows, he says, that (1) does not express the same proposition as (2), and that the paradox of analysis dissolves. He says, in effect, that no plausible interpretation of being the same proposition would permit us to say that an identity is the same proposition as one "in which a non-identical relation is a component".

It seems to me that Prof. Black has made an error. He appears to be arguing that no two sentences can express the same proposition if one expressly mentions a relation which is distinct from the relation expressly mentioned by the other. As a counter-example consider the following. Using Prof. Black's example from arithmetic,

[1] MIND, no. 211, pp. 263-267.

[2] "The Notion of Analysis in Moore's Philosophy," *The Philosophy of G. E. Moore*, pp. 319-342.

suppose I choose to express the proposition that $21 = 3 \times 7$ by the sentence "21 is thrice 7". It is obvious that the two-termed relation of *being thrice* is different from the three-termed relation that holds between 21, 3, and 7, nevertheless "$21 = 3 \times 7$" expresses the same proposition as "21 is thrice 7". And finally, let us construct a translation of (1) which parallels the idiom in which "thrice" is used. Unfortunately there is no parallel idiom in standard English usage which can be applied to the brother-male-sibling situation, but we can give a name to the relation which holds between one relation and another when the first is the second with its domain limited to males. Call this relation "C". Then we may express (3) as follows:

(5) $\quad C(b, s)$

It would not follow that (5) and (3) express different propositions, a false conclusion to which we are led by Prof. Black's principle.

The error in Black's argument is due to his failure to see that the synonymity of two sentences expressing relations depends not only on the relations expressed but also on the terms expressly said to be related.

MORTON G. WHITE.

V.—CRITICAL NOTICES.

Philosophy in a New Key—A study in the Symbolism of Reason, Rite, and Art. By SUSANNE K. LANGER. Cambridge, Massachussetts: Harvard University Press. London: Humphrey Milford, Oxford University Press, 1942. Pp. xiv + 313. $3.50.

THIS review must begin with an apology for its long delay owing to great pressure of work upon the reviewer. I am sorry that a book of such exceptional interest and merit as Mrs. Langer's should have had its official introduction to readers of MIND so long postponed.

The "New Key" is the new recognition of the importance of symbols. Everywhere, the author points out, symbols and their meaning have come into the focus of attention. Science and technology, which at first sight appear to be concerned with sheer observation and sense evidence, are in fact less empirical than they seem, having far more to do with symbols than with sense reports. Sense reports are in the main not of the facts being investigated, but of index needles, revolving drums, sensitive plates—themselves symbols. To yield scientific meaning they need interpretation, expressed as a rule in mathematical equations. Mathematics again, rational, *a priori*, non-empirical, is nothing if not a system of symbolisation, and to pure mathematics science pays unreserved homage. And in philosophy itself—and in literature and logic and psychology—the growing preoccupation, in scores upon scores of works, with the nature of symbolism, is an indication that a new generative idea is active. The philosophical questions derived from the Cartesian tradition, with its dichotomy of inner and outer worlds, are no longer alive; but something else is. "In the fundamental notion of symbolisation—mystical, practical, or mathematical, it makes no difference—we have the keynote of all humanistic problems" (p. 25).

What then is symbolisation? In the first place it is *not*, as some genetic psychology teaches, merely a development of the practical life-preserving response to signals or signs. No doubt man's superiority in the race for self-preservation was first due to his capacity to respond to a wide range of signals. But man, unlike other animals, uses signs not only to indicate things, but also to represent them. Most words are used, not to direct our eyes and ears and noses towards things, but to talk *about* them, and *in absentia*. Such 'signs' are not symptoms of things, but *symbols*. Symbolic activity is indeed often most *un*-biological. The animal's experience is checked realistically, but human symbolism, in words or otherwise, may pickle our errors, confusing and warping our responses. If man is merely a superior biologically adaptive animal, he is extraordinarily inefficient, for the meows of a cat are more efficacious than the many bootless rites and sacrifices which characterise religion and art.

We are therefore driven to reconsider human needs. For Mrs. Langer, man's basic need is the need of symbolisation—seen in dreams, ritual, art, superstition, religion, speech, science. Even the love of talk is significant of man's delight in the sheer expression of ideas. And ritual, as we shall see, is not primarily prescribed for a practical purpose, even of social solidarity: it is rather the desire to symbolise great conceptions. As Freud has recognised, ritual acts are not genuinely instrumental acts, but are performed from sheer inward need.

In Chapter III Mrs. Langer begins her more precise study by an account of the logic of signs and symbols. 'Meaning' is psychological ('I mean'), or logical ('it means'). Meaning is not a quality, nor even a relation. It is a *function*, which is "a *pattern* viewed with reference to one special term round which it centres; this pattern emerges when we look at the given term *in its total relation to the other terms about it*" (p. 55), the term itself—depending upon our interest—holding a key position. The two kinds of meaning, signs and symbols, are now carefully differentiated, the sign (which may be 'natural' or 'artificial') indicating the existence—past, present, or future—of a thing, event, or condition, the symbol *not* evoking actions appropriate to the presence of the object, but thoughts. Symbols "are not proxy for their objects, but are *vehicles for the conception of objects*". Symbols directly 'mean' conceptions, not things. Signs 'announce', but symbols lead us to conceive—though it is obvious that the same entity (*e.g.* a name) may be used sometimes as a sign and sometimes as a symbol. The importance of conception through symbols, as opposed to signification, is well illustrated in Helen Keller's account of the spelling of the word "w-a-t-e-r", as the cool stream gushed over her hand. Here was not merely action, adaptation, but contemplation, characteristic human emancipation. "That living word", Miss Keller wrote, "awakened my soul, gave it light, hope, joy, set it free!"

Both signs and symbols may involve us in *mistakes*. Considering now only symbols, *error* may arise denotatively or connotatively. In denotation—which is the complex relationship of a name to an object—we may apply the name to the wrong object. In connotation, which is the relationship of a name to a conception (used in a wide sense, and not as identical with 'concept') we may be mistaken about the conception. But truth and error arise chiefly in *discourse*. Discourse involves grammatical structure and syntax which in propositions represent (and misrepresent) states of affairs. What matters is the relation of elements, the pattern. Language has obvious advantages over even diagrammatic pictures. For the most part the relations expressed in language are not—as in pictures—symbolised by other relations, but are *named*, just like substantives. 'Brutus *killed* Caesar'. Here the asymmetrical relation is indicated by the order; and case, mood,

tense, etc., give enormously compressed information. (Try to *picture* 'Your chance of winning is one in a thousand'!) Words, again, have the advantage that (*a*) they require almost no physical effort, (*b*) they have no value except as symbols, (*c*) they readily enter into combinations.

Language is discursive; propositions are true or false; and some propositions, but not all, are verifiable. Are we to conclude, with Carnap, Wittgenstein, and many others, that those linguistic utterances which cannot be verified, have no literal meaning, or have literally no meaning at all, represent nothing, are not even true or false, but simply *unthinkable?* Are we to put them on one side as 'emotive', as expressions of feelings or desires, like 'Oh, Oh', whether the 'Oh' of the Spinozistic metaphysics is esteemed valuable (as by Russell) or whether it might be described coldly (though technically) by Wittgenstein as just "senseless"? Mrs. Langer has great respect for the philosophers just mentioned, but their dichotomy of thought-feeling she rejects. As against (what has always seemed to the present reviewer) this presumptuous and inexcusable dogmatism without even psychological justification—for, apart possibly from exceptional cases of 'drugged' emotions, emotion without meaning has no place in modern psychology—Mrs. Langer writes, "Where Carnap speaks of 'cries like "Oh, Oh"', or, on a higher level, lyrical verses', I can only see a complete failure to apprehend a fundamental distinction. Why should we cry our feelings at such high levels that anyone would think we were *talking?* Clearly, poetry means more than a cry; it has reason for being articulate; and metaphysics is more than the croon with which we might cuddle up to the world in a comfortable attitude. We are dealing with symbolisms here, and what they express is often highly intellectual. Only, the form and function of such symbolisms are not those investigated by logicians, under the heading of 'language'. The field of semantics is wider than that of language . . ." (pp. 86-7). Even the eye and the ear have their logic, as the *Gestalt*-psychologists have shown. They are as capable of articulation (of complex combination) as words. On the other hand, they are not discursive, but presentational, simultaneous. There is no break with logic, or rationality, in the strictest sense, in that these presentational forms have *meaning:* but it is not meaning capable of propositional expression. Why, however, should not presentational forms be conceived as media for the symbolisation, for the conception, expression, and apprehension of impulsive, instinctive, and of more deeply human sentient life? Such a notion is not non-rational, or anti-rational, if 'reason' is taken broadly. We have no right simply to assume that ritual, myth, and music have not each their *rationale*—though they may be very different from the *rationale* of discursive propositional thought. This contention is given positive content in four central chapters of Mrs. Langer's book—on language, sacrament, myth, and music.

The primary motive of language, Mrs. Langer holds, is the transformation of experience into conceptions, and not the elaboration of signals and symptoms. Animal noises are expressions and symptoms of their states, signifying their feelings or desires. True language begins only when a sound keeps its reference beyond the situation of its instinctive utterance. Not "Yum-yum", or "My love, my love!" but "He loves me, he loves me not". So, again, Mrs. Langer contends (I am certain rightly) that—though the origins of language are unknown—the *primary* end of language is most unlikely to be that of communication. Rather, once more, language is primarily a vocal actualisation of the tendency to see reality symbolically. Communication depends upon this, and not *vice versa*. This view is supported by the citation of the symbolical 'superstitions', the 'bogeys', 'mementos' of apes, children's babbling and autistic speech (which frequently has no practical meaning, sounds being to the human young interesting on their own account), Helen Keller's awakening by a contemplative and æsthetic, and not a practical, fulfilment. Children—enormously helped and practised by the habit, at a certain stage, of 'useless' babbling—learn to speak by using words to bring things into their minds, not into their hands.

Genetically, language may have originated in ritual. Ritual, in its beginnings non-practical, is accompanied by fanciful sounds, syllables, shouts, song. Ritual dance, shouts, and song, becoming associated with certain events or some central figure, would lead to naming. What was primarily connotative, like 'Hallelujah', would become denotative, through association with objects. Then, because these activities are social, the word would come to have a common meaning, making communication possible. As for the relational structure of language, that is likely to have developed from the central fertile nucleus of a word. Consider the possible propositional outgrowths of the single word 'Out', or 'Cookie' on the lips of a child. These developments are described at some length under 'emendation' and 'metaphor'. Metaphor is of vital importance. By it we both extend our experience by analogy, and later, criticising the analogy for its literalness, form new and more abstract concepts. Our language, according to Wegener, is a rich repository of "faded metaphors".

Now follows a fascinating account of Life-symbols, the roots of sacrament—too complex to summarise. The first thing we instinctively strive to conceive is simply the experience of being alive. Since metaphor is the law of the growth of semantic, for the infant's mind everything soft is his mother, everything that meets his reach is his food. And, as infantile symbols multiply, wish and fantasy grow up together, the mind in its desire for conception producing ideas without number, inextricably mixed up. In such metaphorical thinking the allegorical status of images is not recognised, any more than in the imagery of dreams, the perceived symbols,

interesting in themselves, are distinguished from their meanings. Likewise in primitive adult thought, which is not far removed from dream level, the scarab or the charm or the symbols of life or of death have mysterious significance in themselves. And when natural symbols, such as the snake in the earth, or the bull or the long-lived crocodile, are consciously set up in temples, their symbolic force may be emphasised; and also condensed; the snake may be horned or crowned or bearded, the bull may have wings or a human head. And, once again, the emotion commanded by those *sacra* is not the joy of possessing the advantageous, but the delight in a presented idea. Since the symbol is fused with its meaning, and life or death are efficacious, we wrongly think that a symbol such as the cross is *primarily* valued because it is efficacious.

The contemplation of *sacra* gives rise to motor attitudes—shouting, prancing, rolling on the earth—which are no doubt in the first instance self-expressive, but soon are used to demonstrate rather than to relieve feeling. Its demonstrative intent becomes clearer through contagion, collective activity, the communal act. Becoming stereotyped and formalised, this overt behaviour before *sacra* is ritual, whose function is to develop a tribal or congregational unity of rightness and security. "A rite regularly performed is the constant reiteration of sentiments towards 'first and last things'; it is not a free expression of emotions, but a disciplined rehearsal of 'right attitudes'" (p. 153). And then, since emotional attitudes are linked with the needs of current life, and as *sacra* are primitively regarded as life-givers and death-dealers, they are trusted or feared or placated, as well as revered (*e.g.* the Ark).

The most universal form of ceremonial is the sacrament, the overt form of which "is usually a homely, familiar action, such as washing, eating, drinking; sometimes a more special performance—slaughter, or sexual union—but still an act that is essentially realistic and vital" (p. 159). This is not strange, because the sacramental act must be of a type that is familiar and often repeated. Then, the meaning of the sacrament soon becomes *personalised*. In 'eating' sacraments the meat becomes a host, or the symbol to which rites of supplication and offerings are addressed becomes a "Holy One" or a "Herm", or at a later stage, a god, rising above the *efficacy* of the cult object, to the *ability* of gods, to whom prayer is properly addressed.

From these primitive stages to the belief in the Olympian gods or the Triune God of Christianity is a long step. The last part of Chapter VI is devoted to an account of Jane Harrison's view that it is through *myth* that human conceptions of divinity really become articulated. "A symbol may give identity to a god, a mimetic dance may express his favours, but what really fixes his character is the tradition of his origin, actions, and past adventures" (p. 169). He becomes a personality by his story. Homer, as Herodotus said, gave the Greek gods their names and stations and even their

shapes. "Divinities are born of ritual, but theologies spring from myth" (p. 169).

If ritual begins in motor attitudes, myth begins in fantasy, remaining tacit for long in dreams. The lowest form of story—the 'silly' and 'impossible' story of primitive men or children—is little more than dream, but rather less incoherent: and as the story goes abroad, coherence increases and personal symbols are replaced by more universal ones. But this is not yet myth. Myth, as distinct from 'silly' stories or fairy tales, is not irresponsible individual wish-fulfilment, but is religiously serious, and is taken either as historic fact or mystic truth. The theme tends to be tragic, not utopian, myths become interrelated, forming cycles; the stage is the actual world (Mount Olympus, the sea, the sky), cosmic rather than merely local; the hero has not merely magic powers, but is superhuman or supernatural. The end of myth is not wishful distortion but truth, moral orientation.

The author gives an interesting account—amply illustrated from Polynesian mythology—of how (in her opinion) the heavenly bodies came to be regarded anthropomorphically. The usual cliché, 'personification' is rejected as absurd; no one, even a poor savage, could *mistake* the sun for a man. Mrs. Langer thinks that the identification develops through *legend* and its 'culture-hero'. The culture-hero has a vague complex status, part man, part demi-god. Hiawatha, for example, is both a very human character who is hungry or gets stung, and a being who can take mountain ranges in his stride. Or Mani, the Polynesian demi-god, is the buffoon or naughty boy with heroic and divine qualities. He is Man. Whence, then, did he come? Cosmically, 'out of the Night'; humanly, 'out of Woman'. Very interestingly the Polynesian word *Hine* by itself connotes 'light', or 'falling', 'declining'. In composite words it usually refers to a woman. In Polynesian mythology *Hina* also means the moon. The mixture gives the word a generalising function which is highly significant of the generalised personalities which culture-heroes—and heroines—are. The moon becomes a 'condensed' symbol for Woman. It expresses the whole mystery of Woman, the Great Mother, with her rhythmic phases in the larger of which her body waxes and wanes in life-giving, the smaller time-cycles being those of withdrawal, with its elaborate taboo. It expresses Woman, too, in the moon's inferiority to the sun, and in her cloudy garments. Again, as in the myths life is swallowed by death, and the swallowing monster is ancestor to the life that dies, so it is with the moon. The moon in all its phases symbolises the extraordinary complexity of birth-life-death meanings. So it is not simple personification: Hina is not a symbol of the moon, but the moon is a symbol of Hina, Woman (and all the conception contains). But further, because everything that acts upon primitive man *is* personified, the moon is a person like man himself. Yet it is not a personification of the moon, but a lunarisation of Hina.

(I fear it is quite impossible to condense satisfactorily this part of a subtle argument with its ample illustrations. I am not sure that I completely follow it.) Finally, the processes of personification in their higher and more consistent developments in Greek mythology obviously owe much to the poets. But not everything.

The 'meaning' of art, and in particular of music, is now examined. Art is said to be 'significant' or 'expressive' form: but what does it express? Clearly not propositions, and not *simply* the 'unconscious'—for this in itself affords no criterion between good and bad in art: the significance of art is inseparable from the sensuous 'construct'. Nor are explanations (of music, for instance) based exclusively upon intellect, upon 'survival', upon the 'nervous system', 'sensation', 'emotional response', to be accepted. In particular, the doctrine of 'self-expression' in music is rejected. *Sheer* self-expression (*e.g.* of anger or sorrow) requires no artistic form. And though music may be *used* to relieve emotion, its primary import is semantic, not symptom. It is expository, representative, articulatory, not of thunderstorms or galloping horses, but (in some sense) of the drama of the emotions. Musical structures do logically resemble certain dynamic patterns of human experience; *crescendo, diminuendo, accelerando, ritardando* have some counterpart in mental life. On the other hand, attempts to reduce music to an exact 'language' break down. 'Language' implies vocabulary, syntax, dictionary meanings, translatability, and Mrs. Langer has shown (pp. 94 *seq.*) that no art strictly possesses these. Tones are *not* 'words', harmony is not 'grammar', thematic development is not 'syntax'. And, most important of all, the meaning of music is entirely bound up with its articulation in musical form: in spite of the efforts of Schweitzer and Pirro to trace the 'emotional vocabulary' of Bach, musical semantic can never be isolated from its form. "The analogy between music and language breaks down if we carry it beyond the mere semantic function in general . . ." (p. 232). Yet clearly it does have this semantic function, and those who, like Hanslick, in their rightful anxiety to deny that music is crudely representative, that it has fixed meanings, assert that 'music means itself' are merely evading the issue. The positive truth underlying their assertion is (I should say) that meaning is 'musical', *i.e.* that in all its concreteness meaning is individual and inseparable from the music. It is only in a very general and very inaccurate way that the independent 'movements' of mental life can be said to 'correspond' to the movements of music. "Articulation is its life, but not assertion; expressiveness, not expression" (p. 240). A 'programme' is a crutch, but we need not make a virtue out of using a crutch. And its primary intent—like that of other presentational symbols—is insight, not communication.

This central idea is worked out and applied in Chapter IX to the 'genesis of artistic import'. Sailors' cries, flail strokes, drums, cradle-rhythms, work-rhythms, these and the rest may be incipient

'themes' of music, but do not enter into music proper, and the 'genetic fallacy' is particularly fatal to symbolism. Again, the 'models' of the plastic arts, and the rhythm and words which are the 'models' of music are left behind. Artistic 'truth' is not correspondence to a model, and "to understand the 'idea' in a work of art is more like having a new experience than like entertaining a new proposition" (p. 263).

The last chapter, "The fabric of meaning", too complex to summarise here, contains interesting reflections on the effect upon man's life of the artificiality of civilised city life. A few individuals here and there still live in the realm containing the ultimate symbols (*e.g.* the sea, the air, scientific truth, the great 'Cause') which give the orientation without which man cannot live or be free. The modern mode of living—in apartments—or of working is too *thin* to be any longer a sphere of ritual. So the modern mind is tossed about, seeking solace in hybrid religious mysteries, ideologies, bringing strife, war. (And here one might say that an appropriate —indeed a necessary—supplement to this book on symbolisms, would be a treatment of the symbolism, not of *primitive* but of *developed* religion. Such a supplement would have to be at least another book. Perhaps Mrs. Langer would be prevented from writing such a book by her *Confessio Fidei?* (*circa* p. 40)).

I have not left myself much space for comment. But first I would urge philosophers to read this most important book, which is far richer and far more penetrating than any bald summary can indicate. Sometimes indeed it is almost overladen with ideas and illustrations, and I have not always been quite sure of the exact import of the arguments. (This has, of course, an advantage; it compels one to re-read.)

With her main theses, or at any rate with her conclusions, I am nearly always agreed. But a certain caution is needed in reading her introductory chapters lest they should appear to lead to the wrong conclusion. Man's "*basic* need", we are told, is symbolisation (p. 41). "Symbolisation is the *essential* act of mind" (*ibid.* quoting A. D. Ritchie). "Speech is . . . the normal *terminus* of thought" (p. 45). Ritual, like art, is essentially "the active *termination* of a symbolic transformation of experience" (*ibid.* All italics mine). These sentences, taken by themselves, would suggest symbolisation to be the chief and ultimate end of man's life. Again, sometimes in the earlier part of the chapter on Music one gets the impression that the correspondence of symbolic discursive propositions is being employed as a type of explanation for the 'truth' of music. The *tempi* of music 'correspond' with the dynamic patterns of emotional life. Yet this is not, I think, her final meaning. More ultimate than symbolisation, or the correspondence of symbols, is orientation, adjustment to reality. Of ritual she says (p. 49), "its central aim is to symbolise a Presence, to aid in the formulation of a religious universe". Or again, of rituals, "their primary

achievement is not entertainment, but *morale.* They are part of man's ceaseless quest for conception and orientation. . . . Ritual is the most primitive reflection of serious thought, a slow deposit, as it were, of people's imaginative *insight into life*" (p. 157, italics mine). Or "to understand the 'idea' in a work of art is . . . like *having a new experience* . . ." (p. 263). Not symbolism, or correspondence, but adjustment, new insights, *through* symbolism.

I would go on beyond Mrs. Langer and suggest for myself that the recognition of this may turn out to be of importance not only for presentational but, reflexively, for discursive thought itself, and not only for the theory of knowledge, but for speculative philosophy.

It is convenient, and right, when discussing the 'meaning' of propositions, to attend to logical, rather than to psychological meaning, to 'The sentence means', rather than 'I mean'. For here 'I' = 'we' = 'all of us who speak the same language', and *what* the sentence means is something accessible to different minds. All the same, 'The sentence means' is an incomplete statement; it is, whether we notice it or not, the expression or instrument of a living active mind in a mental act of cognition, which is one mode of the mind's orientation to reality. This cognitive awareness is something with which every reflective person is introspectively acquainted, but it cannot be defined, and 'orientation' is only a metaphor. Is it then 'true' or 'false'? This is partly a matter of terminology, but personally I should prefer [1] to speak of adequate or efficient cognising, or *vice versa.* It is impossible, however, to judge whether such cognising has been efficient or inefficient until it is expressed in discursive symbolic forms which are 'true' or 'false', and the criteria of truth and falsity are again further questions. An 'efficient' cognitive acquaintance with a Brutus-Cæsar situation can only be judged 'efficient' if its expression 'Brutus killed Cæsar' is found (by various tests) to be 'true'. For this reason, and perhaps for others too, the truth of the proposition is what we normally attend to, ignoring the fact that it is the outcome of a piece of cognitive acquaintance. Nevertheless we may be led into errors if we allow ourselves to assume, as we often do, that *knowledge* can be described in terms of the 'correspondence' or 'analogy' which apply to propositions, instead of regarding propositions as the outcome of a living, intimate, indefinable act of direct acquaintance with the independent real.

In pure matter-of-fact propositions where there is agreement about terms, this distinction is of no direct interest. It may be of some, or of much, importance where there is dispute about meanings: 'What do you mean by . . . ?' If there is any difference about sheer matters of fact, of common sense, in history, in scientific truth, the difference may be settled easily by elucidation and agreement. But if what is referred to is, say, an æsthetic object, in-

[1] I did suggest a different view in a book published in 1923.

dividual and unique and not capable of being symbolised otherwise than in its own medium, then, if there is to be profitable conversation about it, it is necessary that the participants should have had personal cognitive experiences of that object, experiences involving some feeling or even emotion. The propositions that they make about the object, though they will themselves be discursive and not æsthetic, will involve a personal living æsthetic experience which is in sharp contrast to the colourless 'cognitive' experience of the Brutus-Cæsar situation. The characteristics of the personal acquaintance and all they involve are important here as they are not in the trivial or obviously true or false propositions about which philosophers so often dispute. The same would be true of propositions about religion.

This has important bearings on philosophy—at least upon 'synoptic' philosophy in the large traditional sense, which I will not here assume to have been ignored out of existence. The material of philosophy in this sense consists of every kind of content of experience, moral, religious, æsthetic, as well as objects of sense-perception or science. If the assumption that the æsthetic and the religious are merely 'emotive' is shown to be dogmatic and superficial, if they possess genuine meaning, and meaning which is not adequately expressible in propositional form but which can only be known by personally entering into them, then the philosopher who either cannot or will not enter into them is so far disqualified as a philosopher, or disqualifies himself. It is not simply that his private personal experience is limited, but that because (as we are now assuming) these things are *meaningful* and not mere matters of feeling, the material for his synoptic philosophy is fundamentally deficient and a philosophy which insists on confining itself to interest in situations which are clearly expressible in propositions is shirking an essential part of its task.

It is true that all philosophy does not set out to be 'synoptic'; perhaps most does not. There is unlimited work of detailed analysis to be done. But the distinction between 'critical' and 'speculative' philosophy is—or should be—one of convenience and emphasis; quite certainly it is not ultimate. Every good philosopher knows, not only that it is impossible to draw hard and fast lines round the special problems which he is investigating, but that the pursuit of the apparently irrelevant is often most fruitful, and that ultimate judgments are liable to be involved at any time, affecting, it may be, the solution of the particular problem. Philosophy is not one of the sciences. This being so, the knowledge gained from reflection upon the subjects of art and religion will certainly affect the treatment of other contents within the 'synopsis' and will modify and, it may even be, transform, the synopsis as a whole.

It is not suggested that the expressed synoptic philosophy will therefore itself be 'æsthetic' or 'religious'. The language of philosophy is propositional, and its aim is the utmost clarity and

definiteness of which the subject-matter will allow. On the other hand, since the æsthetic and the religious are not adequately describable in words, a philosophical treatise may sometimes have to use æsthetic or religious language even to indicate more nearly what it is talking about: and if consideration of verbally inexpressible religion and art may affect or transform the synopsis as a whole, then even the cool language of philosophy may at times be such as cannot fully be comprehended by those who cannot or will not enter upon æsthetic or religious experiences. And, as Plato knew, there may be matters (not, in the most technical sense, 'nonsense') which, as yet, can only be hinted at in the language of metaphor or myth.

Finally, if even the simplest proposition is the expression of an act of acquaintance, and an 'orientation', and if art and religion are in a more important sense personal insights, synoptic philosophy will also be a personal insight, an orientation, something like, perhaps, the orientation of what has sometimes been called wisdom. This insight will express itself in systems of propositions, though it is at least doubtful whether it can ever do so completely.

LOUIS ARNAUD REID.

Philosophical Commentaries, generally called the Commonplace Book. By GEORGE BERKELEY, Bishop of Cloyne: An *editio diplomatica* transcribed and edited with introduction and notes by A. A. LUCE, M.C., D.D., Litt.D. Thomas Nelson & Sons, Ltd., 1944. Pp. xlii + 485. £3 13s. 6d.

DR. LUCE has provided us with a really satisfactory edition of Berkeley's *Commonplace Book*. It has already been described by various reviewers as the definitive edition and no doubt that description is true. Whether it is definitive in the sense of giving us a wholly correct text is more than anyone can say at the present moment. The manuscript (Brit. Museum, Add. MS. 39305) is now inaccessible and has been so for some years. When after the war it becomes accessible again it will be the task of some scholar to compare the original with this present edition. I shall be surprised if he will find many (or indeed any) errors; but Dr. Luce himself would, I am sure, welcome the independent check, particularly since, I suppose, he also has had to go to press without having seen the MS. for the last three or four years. No doubt he was well forward with the work by 1939, and had already fixed the text. At present, however, for the reason given, it is impossible for a reviewer to say whether the edition is textually correct or not. One can only express one's faith in Dr. Luce's Berkeleian scholarship.

The *Commonplace Book* is a difficult work to edit. It is to be feared that those who rely on the earlier editions have but a poor conception of the work. So much depends on the actual lay-out of

the MS. as everyone acquainted with it will admit. Perhaps the best edition would have been a photostat copy, although it would have been difficult to read. Dr. Luce gives us a diplomatic edition, that is to say, the text is *printed*, but the position of the entries in relation to the page and to each other, and particularly in relation to entries on the opposite side of the page, the *verso*, is retained. In this way the reader gets an excellent idea of the text and of the manner in which Berkeley set out his thoughts. Such an edition is likely to be costly; produced in the magnificent way in which this one has been produced it becomes very costly and one finds something approaching an apology in the preface. Is the *Commonplace Book* "entitled to the honour of an *editio diplomatica*"? Most philosophers I think will agree with Dr. Luce's answer and will feel glad that we have at last so excellent an edition of this exceedingly important as well as fascinating work. It is pleasant to record also that this definitive edition has come from Berkeley's own college, and there is a note of justifiable pride in Dr. Luce's remark: "Berkeley's work has now been edited where he wrote it", and edited, one may add, in such a way that it is unlikely that anyone else will want to do the job again for very many years. Perhaps some years hence Dr. Luce himself and the publishers will find it worth while to produce a cheap edition on less sumptuous lines for the use of the student, but any such edition would have to be based on the present one.

Dr. Luce is unhappy about the title, *Commonplace Book*, which Fraser, who first published the book, gave to it in 1871 and by which it has since been known. A commonplace book consists of random jottings, a collection of thoughts from various writers which have seemed to the author important and which perhaps led him to other thoughts which he also records. Now the work here edited is certainly not a collection of other people's thoughts. It is a commentary, Luce thinks, "upon a complex argument for immaterialism which was present in outline in Berkeley's mind for some time before he began to fill the notebooks. With that argument in view throughout, Berkeley here comments upon his own theories, upon the difficulties they involve, upon the books he read, and upon his own literary plans." In general this seems an adequate account of what the book is about and perhaps the title *Commonplace Book* is misleading. Dr. Luce re-names the work *Philosophical Commentaries*. A commentary, however, suggests to me something fuller than we find here, *viz.* exposition and comment upon a text before us. Dr. Luce actually makes the suggestion that there may have been a written text before Berkeley when he wrote, his first statement of immaterialism, completed before he began the *Commonplace Book*. This is certainly possible. Even so, the *Commonplace Book* which has nowhere a direct reference to that earlier work can hardly be described as a commentary upon it in the ordinary sense. If we reject the title *Commonplace Book*, what of *Philosophical Comments* or *Philosophical Notes*? I wonder, however, whether it is not now

too late to change the title. Even Dr. Luce lacks the courage of his convictions in this matter, for throughout his notes on the entries he uses Fraser's title and not his own in referring to the work.

And it is obvious that Fraser's title is not altogether wrong. In that age (prior to the appearance of encyclopaedias) it was the fashion for each scholar to compile his own commonplace books. Locke wrote *A New Method of a Commonplace Book.* And it does seem to be the case that Berkeley had in mind something of the kind in filling the two notebooks, which together make up the MS. He does refer to other authors frequently and sets out their views. Dr. Luce's point, however, is that the whole book is compiled around a theme. These are not random jottings. They are random in the sense that the respective entries do not form one continuous argument. Some of the consecutive entries do deal with the same subject-matter, others of them do not. But all the time there is an underlying continuity, and as one reads through the work the underlying purpose of the whole becomes more and more evident. I think that Dr. Luce's argument here is sound, and it follows that this is no ordinary commonplace book. Dr. Luce's own description of the book on p. xxxiv is excellent : " The notebooks were companions of Berkeley's reading and writing ; in them he has noted down thoughts, his own and those of others, which were likely to assist his writings ; here he records his literary plans, giving himself advice on style and language ; here he has noted difficulties in his thesis ; here he has hammered out arguments, revised views, examined technique, fixed his terminology, and (towards the end) drafted sections ".

If, however, the *format* of the work is due in part to the fashion in commonplace books at the time, it is also in part due to another cause not perhaps sufficiently emphasised by Dr. Luce. The MS. begins with a set of Statutes for a Society. There then (folio 102) follow certain queries about Locke's *Essay.* On the next page are the rules of (apparently) a second society to " discourse on some part of the new Philosophy ". At the back of this notebook (folio 164 v.) are further queries on Locke's *Essay.* What are these queries ? Dr. Luce thinks that the queries on f. 102 are questions propounded to candidates for a degree. " They constitute a *disputatio ;* they are what we should now call an examination paper on Locke's *Essay.*" It may be so, although some of the entries here make very peculiar examination questions. But why should not they and the queries on the reverse of folio 164 be subjects for discussion at one or other of these societies ? And if they are, may we not suppose that the entries of folio 104, " One eternity greater than another of *ye* same kind ", " In *wt* sense eternity may be limited ", and so on, are also queries for the society. Whether these latter were ever debated in the society or not, they were clearly debated in Berkeley's mind. Entries prefaced *Qu :* appear frequently in the early part of the work. Did Berkeley begin by compiling queries on the new philosophy (of Locke) for the societies and then proceed in the *Commonplace*

Book to work out the points himself? This would explain his use of the societies' notebook for his own private reflections. Thus the work would be not merely a collection of noteworthy thoughts by other authors, that is to say, a commonplace book, but also a collection of queries about the "new" philosophy of Berkeley's day and a commentary on this philosophy, exposing its difficulties and weaknesses and, on the constructive side, seeking a solution which is found in immaterialism and the New Principle. It is thus a commonplace book, a commentary on contemporary philosophical problems and a commentary on Berkeley's own proposed solution, all combined in one.

Dr. Luce proposes a new system of numbering the entries and it is an improvement on previous editions. In particular Berkeley's later comments (on the *verso*) on entries on the *recto* are given the same number as the *recto* entry but *a* is added. If the entry on the *verso* has no reference to anything on the opposite side of the page, as is sometimes the case, Dr. Luce gives it a number of its own. (Incidentally, may I suggest the possibility to Dr. Luce that the entry he numbers 362a should be 363a, and the present 363a become 363b?) Dr. Luce is surely correct in placing 378 ff. where he does place them. My only criticism in respect to the numbering is that I should be inclined to include the entries on the reverse of folio 164 within my enumeration and also the "Queries" of folio 102. Berkeley has a marginal sign-system and he does not apply it to either of these sets of entries. Dr. Luce contends that for this reason they should not be included in the *Philosophical Commentaries*. But on this principle he should exclude the other occasional entries such as 406, 537, 811, etc., which have no sign alongside them. Furthermore, we now have the inconsistency of an entry (149) on folio 121 being included in the *Commentaries* whereas the identical entry on folio 164 reverse is excluded. For purposes of reference it would surely have been better to include all these entries in one's enumeration. Dr. Luce's explanation of the signs in the MS. is excellent. I do not feel sure that the plus sign + always denotes that the entry thus marked is not to be used in the published works. Nor is the erased S always a sign for Space. In 46 and 47 it seems to stand for Sensation, in 89 (and perhaps 50 and 51) for Substance.

I now turn to problems of the chronological order of various parts of the MS. To begin with a small point, it would obviously be a mistake to say that the numerical order of the entries in this edition is invariably the order in which they were written, for there are those insertions on the *verso* which do not refer to an entry on the *recto*. Thus it is unlikely that 98 was written before 99, as I think Dr. Luce would agree. The important question of chronology, however, is that as to the order in which the two notebooks composing the MS. were written. As will be known to my readers these two notebooks were bound together after Berkeley's death. Fraser published them in the order in which he found them. Lorenz, however, suggested

that the second notebook as bound, notebook B, was in fact the first composed by Berkeley. Most scholars since Lorenz have accepted his suggestion, and this is the order which Dr. Luce also follows. He discusses the matter very fully in his introduction (pp. xvii ff.) and makes out a very strong case for holding that the Fraser supposition, namely, that the whole of notebook A was filled before notebook B was begun, was unsound. I wish, however, that he had examined more fully the case against the Lorenz theory, that is to say, against the view that the whole of notebook B was filled before notebook A was begun. For there is always the third alternative that these notebooks were, for a shorter or longer period, used simultaneously. If I were compelled to choose between Fraser and Lorenz I should choose Lorenz, but I should do so unhappily because, though I agree that in the main B is earlier than A, neither internal nor external evidence proves that the beginning of notebook A is later than the end of notebook B. In other words there is a large block of entries in the middle of the text, a hundred entries or perhaps two hundred —one really cannot say how many—of which it would be wrong to hold that any one found in notebook B must be prior to any one found in notebook A. There is also the singular fact that a whole series of entries (351-358) which appears six folio pages before the end of notebook B re-appears with certain minor changes on the sixth, seventh and eighth folio pages of notebook A (415-424). Did Berkeley fill these first into notebook B and then fill them in again into notebook A, having forgotten that he had already included them in B? It is impossible to say. There seems to be no reason for supposing that he filled them into both books at the same time. But if one series was written before the other we cannot say which was written first. I wish Dr. Luce had paid more attention to the third alternative which rejects both the Fraser and the Lorenz theories as they stand. He does, in passing, touch on the matter in a note to p. xvii, but it deserves far more attention. (Incidentally what light do these duplications throw upon Berkeley's method of composition when writing the *Commonplace Book*? Was it his practice to jot the thoughts down on separate slips of paper as they came to him and later write them into his notebooks? Perhaps he did this sometimes, for instance, when thoughts came to him in discussion with his friends or when walking out alone. At other times he no doubt transcribed them direct into his notebooks.)

As to the date of the work, that is to say, of what is really important in the work, beginning with folio 104 of notebook B and ending with folio 94 of notebook A, I am glad to find that Dr. Luce agrees with the date I suggested in MIND (October, 1931), namely, mid-1707 to the second half of 1708. The evidence for this set of dates is very strong, as Dr. Luce makes clear. I am not as sure as Dr. Luce is that the date August 28th, 1708, at the end of notebook A, is Berkeley's own dating of the completion of the work. I think it refers in the first place to the "Adventure"—whatever that was—though the

"Adventure" might have coincided with the end of the work for Berkeley and marked its end for him. Other evidence, however, makes it highly probable that Dr. Luce and I are correct in holding that folio 94 had been written well before the end of 1708.

In re-reading the *Commonplace Book* on this occasion I was struck by an interesting point which I had failed to notice earlier. Immaterialism is cradled in its pages but it does not come to birth in them. As early as entry 19 we find Berkeley talking of the "immaterial hypothesis" as something already present in his mind. Yet these pages do witness the birth of "the New Principle". The entries on folio 144, 145 (entries 270, 279, etc.) suggest that he was then for the first time grasping the notion that *esse est percipi*, and marvelling that he had not grasped it earlier. In that case we can hardly identify immaterialism with the New Principle. His reflections on the philosophy of Locke and Malebranche had led him to deny the belief in matter, or at least to contemplate this denial, before he began to fill these pages, but the more positive doctrine, *esse est percipi*, came to him later. In other words, pre-270 we have immaterialism but no New Principle. In the light of this reflection entry 80 becomes interesting with its reference to bodies "by them meaning combinations of powers in an unknown substratum". "The 'unknown substratum'", comments Dr. Luce (p. 339), "is spiritual substance." But is it? It was so after he had accepted the view that *esse est percipi*, but at this stage the evidence suggests that he had not accepted that view. Do we find Berkeley here at the parting of the ways—immaterialist and yet not at this point idealist? It is an interesting speculation. What would have happened if he had held to his immaterialism without proceeding to idealism?

The editor's notes on the entries are admirable. Some of the entries are difficult to understand, but the notes throw a flood of light on them which is most helpful. Dr. Luce was fortunate in having the assistance of Mr. E. J. Furlong in dealing with the mathematical entries. Even with the help of Dr. Luce's notes I still fail to understand one or two of Berkeley's more cryptic utterances, but speaking generally this edition makes the entries intelligible in a remarkable way. We are very much indebted to Dr. Luce for the fullness and helpfulness of these comments. A reader who works through the text using Dr. Luce's introduction and comments to guide him is likely to gain a very thorough knowledge of it. I have a minor criticism to make, namely, that never at any point, so far as I have noticed, does Dr. Luce make reference to the excellent work of Benno Erdmann *Berkeleys Philosophie im Lichte seines wissenschaftlichen Tagebuchs* (1919). I found this an excellent commentary on the *Commonplace Book*, I might almost venture to say the best before this edition appeared. It is a pity that it is nowhere mentioned here.

After all the mishaps which have occurred in the editing of this text, it is good to be able to recommend this masterly edition. Dr.

Rudolf Metz once wrote an article in *Kant-Studien* (1926) entitled *Berkeleys Philosophisches Tagebuch* giving the story of the *Commonplace Book* up to date and of its editing in England, France, Germany and Italy. The story which he had to relate was a very strange one —blunder after blunder, most of them finally attributable to Fraser's almost scandalous editing. Since Metz wrote there has appeared the unfortunate Johnston edition. Now Berkeley's own college, as is fit and proper, gives us at last a sound edition. Before we can say that the text is fixed once and for all we must see the original MS. again. But of the rest of the work we can say already that as a commentary on the *Commonplace Book* it is never likely to be bettered.

We ought to congratulate the publishers also on producing in spite of all their present difficulties what must be one of the most beautiful and magnificent books published in this country during this war.

One final complaint. Why are apologists for Berkeley, such as Dr. Luce and Professor Dawes Hicks, so anxious nowadays to present his early philosophy as wholly sensible and non-shocking? After all the "New Principle" was shocking in 1710 and it is shocking still to most people when they first become acquainted with it. But Dr. Luce wants us to believe that the Berkeleian philosophy as revealed in the closing pages of the *Commonplace Book* and in the *Principles* is "a sober philosophy of sense and spirit, which any theist might hold" (p. vii). I doubt whether even Dr. Luce can ever make the young Berkeley completely and wholly "sober". But why try to do so?

R. I. AARON.

VI.—NEW BOOKS.

The Philosophy of Thomas Jefferson. By ADRIENNE KOCH. New York: Columbia University Press; London: H. Milford, 1943. Pp. xiv + 208. 16s. 6d.

The philosophical tradition that found its fullest expression in the writings of William James was more truly endemic than any that has ever flourished in Modern Europe, but it was not altogether an indigenous growth. Its roots are to be found in the confident empiricism that inspired Locke to apply to the moral sciences the same method as Harvey and Newton had used so successfully in the physical. Whatever may have been Locke's shortcomings in pure epistemology, the success that his philosophy enjoyed in the sphere of politics was great and beneficent. For this reason, we cannot hope to understand the remarkable influence of Thomas Jefferson in the modern world, unless we first understand his place in the history of the empirical movement. The present study, which covers this subject quite satisfactorily, is divided into three sections. The first deals with Jefferson's *Ethics* (pp. 1-43), the second with his *Philosophy*

and Ideology (pp. 44-112), and the third with his *Theory of Society* (pp. 113-185).

Jefferson's ethical position might be summed up by saying that he began by accepting the doctrine of the Moral Sense School that conscience and benevolence are innate in human nature, and that he reconciled the apparent conflicts between ethical intuitions by a doctrine of the individual development and social progress of the moral faculty, which led in the long run to a utilitarian standard in regard to the rightness of actions. Conscious of the shallowness of utilitarianism as an account of why men act rightly, he reverted in middle life to the teaching and example of Jesus for the highest ideal of character. The best men, he maintained, are those who perform right acts out of spontaneous affection, rather than from fear of opinion, or even abstract convictions.

In philosophy, Jefferson was a positivist with an unusually clear conception of the scope of the empirical method and the limitations it imposes on the human mind. If he believed with Dugald Stewart in the possibility of a positive science of mental events, he agreed with Destutt de Tracy that psychology must be regarded as a branch, not of metaphysics, but of zoology. His proofs of our knowledge of the existence of the self and the external world are very much in the same terms as those of James and Stout, but they were inspired by the French ideologists, rather than by Locke himself. On the other hand, his scepticism, even as a Deist, regarding the existence of purely mental substances is in the English empirical, as well as the French ideological, tradition.

It was in his theory of society that Jefferson's empiricism achieved its greatest triumph. His doctrine of natural rights was based upon what he believed to be well-founded psychological observations. Men have the same faculties and desires; they are therefore free and equal, and have an inherent right to 'life, liberty, and the pursuit of happiness'. From this general postulate as to the aims of human nature, he infers, not by pure logic, but by empirical observation, that man has need for society, for self-government in society, and for those democratic fences, like Habeas Corpus, Trial by Jury, and Free Expression, which are necessary to preserve the original aims of association. He is particularly wise in the stress he lays upon the futility of representative government, unless it is accompanied by direct self-government in the wards or parishes of a community.

If, in the field of politics, Jefferson gathered the fruits of experience more successfully than any man of his generation, it was because he came to the task inspired by two tenaciously-held ideals. Firstly, he believed that education, particularly education in the facts of history, made men morally better, and wiser in the management of their affairs. Secondly, he believed that man was happiest in an agrarian state, and that republicanism can never be secure unless it rests upon a system of popular landed proprietorship. Modern democracies seem to have accepted the first of these ideals, but to be rejecting the second in the face of the remarkable achievements of mass-production in industry and agriculture. It looks as if Jeffersonians must either convert the world to their sane, but difficult, ideal, or allow themselves to be swamped by the advocates of technological efficiency. We sincerely trust that they will choose the first alternative, and, if so, that many of them will read Dr. Koch's interesting introduction to the subject.

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Received also :—

O. C. Quick, *The Gospel of the New World : A Study in the Christian Doctrine of Atonement*, London, Nisbet & Co., Ltd., 1944, pp. xiv + 119, 6s. 6d.

D. D. Runes, ed. by, *The Dictionary of Philosophy*, London, G. Routledge and Sons, 1944, pp. 343, 27s. 6d.

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W. A. Myers, *The Spinoza-Hegel Paradox*, Ithaca, Cornell University Press, 1944, pp. xiii + 95, 10s. 6d.

H. A. Hodges, *Wilhelm Dilthey : An Introduction* (International Library of Sociology and Social Reconstruction), London, Kegan Paul, 1944, pp. x + 174, 10s. 6d.

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Minerva : Revista Continental de Filosofia, Año 1, Vol. 1, Mayo-Junio, 1944, Buenos Aires, pp. 90, Annual Subscription $4.00 or £1. [A bi-monthly.]

L. S. Eby, *The Quest for Moral Law*, New York, Columbia University Press ; London, H. Milford, 1944, pp. viii + 289, $3.25, 22s.

W. H. Sheldon, *Process and Polarity*, New York, Columbia University Press ; London, H. Milford, 1944, pp. xvi + 153, $2.00, 13s. 6d.

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